VOLUME 97 • NUMBER 3 • JUNE 1992

The American Historical Review

AMERICAN HISTORICAL ASSOCIATION



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In This Issue

This issue begins with an *AHR Forum* in which **Paul W. Schroeder** challenges the venerable notion that a "balance of power" accounts for the success of the Vienna settlement of 1815. He contends that the distribution of power was not balanced but hegemonic. An overall shared hegemony of the two world powers, Great Britain and Russia, combined with a set of sub-hegemonies on the Continent (in Germany, Italy, and elsewhere), formed the working basis of the system. Both the goal and the achievement of the peacemakers was political equilibrium, which, in Schroeder's interpretation, means that the settlement, while resting on hegemony in terms of power, provided a balance in terms of independence, obligations, status, and security through an international system built on a consensus about means and goals and bounded by law.

Enno E. Kraehe agrees with Schroeder's rejection of many traditional explanations for the stability of the Vienna settlement and with his analysis of the emergence of two superpowers, but Kraehe denies that the superpowers exercised a joint hegemony over Europe or the world. In Kraehe's view, the relationship was adversarial and constituted a bipolar balance of power. The talk at the time about "political equilibrium" he regards not as a sign of respect for law but of merely traditional balance of power thinking, which was not as predatory as Schroeder assumes.

Robert Jervis, a political scientist, points out that people in his field conceive of the balance of power as a system maintained by the conflicting interests of states acting in their narrow self-interest. When other states have to fight a large war against a state that seeks to dominate, the balance system is undermined, and the closer ties among many of the states, plus new diplomatic structures, increase the opportunities for cooperation, that is, for a concert system. Schroeder's view represents a challenge to the major positions in political science because he sees the changes involved in moving from a balance to a concert system as much greater, including a transformation in the conception of self-interest, in values, and in beliefs about the international order.

Wolf D. Gruner accepts Schroeder's general conception but not his terminology. Gruner feels that a better characterization of the settlement would be as a multipolar balance of power or "reformed balance of power system," and he brings out some aspects of the relations among the small and middle-sized German states to illustrate his point.

Paul Schroeder responds to his critics and adds some comparisons to the Cold War era.

Kathleen Canning offers a theoretical and historical examination of the concepts of class and class formation from the perspective of gender. She analyzes the destabilization of the concept of class through its encounters with the concepts of gender, race, and ethnicity, a result of the growing interest of labor historians in culture and language. Her study of male and female textile workers in Wilhelmine and Weimar Germany criticizes the current models of class formation and points to the difficulties of integrating gender into existing theoretical constructs. She investigates the importance of pride and honor, morality and virtue, bodies and sexuality in shaping women's work identities, and her examination of encounters between men and women in the textile union reveals how female activists were able to transform the politics of class.

Jacqueline Goggin looks at the struggle of women in the American historical profession during its first half-century and tells a compelling story of the difficulties they confronted in gaining recognition and respect for their scholarly achievements. Unlike women in some other professions, whose activism declined after the success of the suffrage movement, female historians intensified their efforts to achieve professional equality with men after 1920. By the 1930s, women were articulating a sense of themselves as an oppressed group and mounted collective protests against sexual discrimination in the profession. They formed the Berkshire Conference of Women Historians in 1930 to promote their scholarship and professional advancement, and as a vehicle for protest. This period also saw their first efforts at writing and teaching women's history. An important result of their work was the election of Nellie Neilson as the first woman president of the AHA. Surprisingly, these hard-won gains were lost by the 1950s and have only been recouped in recent years. We hope to feature a second article on the progress of women in the profession since the Rose Report of 1960 in an issue later this year.

Yuri L. Bessmertny, a Russian specialist on the history of the European Middle Ages and recent visitor at the Princeton Institute for Advanced Studies, reports on the attempted coup d'état in Russia last August and its meaning for him and his colleagues in the history profession. His method is a retrospective on his life and the condition of medieval studies in Russia from the Stalin era to the present.

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AHR Forum

Did the Vienna Settlement Rest on a Balance of Power?

PAUL W. SCHROEDER

THE QUESTION POSED by the title of this essay must appear a bit unreal. However much historians have differed in interpreting various aspects of the Vienna settlement and the nineteenth-century international system founded upon it, they have never doubted that these included a balance of power as an essential ingredient.[1] Irrefutable evidence seems to come directly from the peacemakers at Vienna themselves; in everything from official treaties to private letters and diaries, they spoke of peace and stability in terms of a proper balance ("juste équilibre") achieved by a redistribution of forces ("répartition des forces"), or in similar balance of power phrases.[2] Not only did their language seem to make the balance of power a vital goal and working principle of the settlement but so did their conduct and the outcome of their efforts. What else were the statesmen at Vienna doing if not restoring a balance of power in Europe by redistributing territories and peoples? What can account for international peace and stability after 1815 if not that the European balance of power was restored after a generation of French revolutionary expansion and Napoleonic imperialism, this time supported and strengthened through a system of alliances, treaty guaran-

[1] The standard work is still Karl Griewank, *Der Wiener Kongress und die europäische Restauration, 1814–1815*, 2d edn. (Leipzig, 1954). Other important treatments include C. K. Webster, *The Foreign Policy of Castlereagh, 1812–1815* (London, 1931); E. V. Gulick, *Europe's Classical Balance of Power* (Ithaca, N.Y., 1955); H. A. Kissinger, *A World Restored* (Boston, 1957); Maurice Bourquin, *Histoire de la Sainte-Alliance* (Geneva, 1954); and Enno E. Kraehe, *Metternich's German Policy*, Vol. 2, *The Congress of Vienna 1814–1815* (Princeton, N.J., 1983). See also Alan Sked, ed., *Europe's Balance of Power 1815–1848* (London, 1979), especially the essays by Douglas Dakin and F. Roy Bridge.

[2] The main published documentary sources are Leonard J. B. Chodzko (pseudonym Comte d'Angeberg), ed., *Le Congrès de Vienne et les traités de 1815*, 4 vols. (Paris, 1863); Russia, Ministry of Foreign Affairs, *Vneshniaia politika Rossii xix i nachala xx veka: Dokumenty rossiiskogo Ministerstva inostrannykh del*, series 1: 1801–1815, vol. 8 (Moscow, 1967); A. A. Polovtsov, ed., *Corréspondance diplomatique des ambassadeurs et ministres de Russie en France et de France en Russie avec leur gouvernements de 1814 à 1830*, Vol. 1 (St. Petersburg, 1902); Klemens Lothar Wenzel Fürst von Metternich-Winneburg, *Aus Metternichs nachgelassenen Papieren*, Prince Richard Metternich, ed., 8 vols. (Vienna, 1880–84); Robert Stewart, 2nd Marquess of Londonderry, *Memoirs and Correspondence of Viscount Castlereagh*, Charles Vane Marquess of Londonderry, ed., 12 vols. (London, 1850–53); Charles Maurice de Talleyrand-Périgord, *Corréspondance diplomatique*, 3 vols. (Paris, 1889–91); Talleyrand, *Mémoires du Prince de Talleyrand*, the Duc de Broglie, ed., 5 vols. (Paris, 1891–92); Arthur Wellesley, 1st Duke of Wellington, *Supplementary Despatches, Correspondence, and Memoranda*, 15 vols. (London, 1862–80); Karl Robert Count Nesselrode, *Lettres et papiers du Chancelier Comte de Nesselrode, 1760–1850*, Count A. de Nesselrode, ed., 11 vols. (Paris, 1904–12); Wilhelm Oncken, *Österreich und Preussen im Befreiungskriege*, 2 vols. (Berlin, 1876); and C. K. Webster, ed., *British Diplomacy, 1813–1815* (London, 1921).

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tees, and Concert diplomacy?

The balance of power interpretation of the Vienna settlement appears so obvious that a challenge to it is likely to be understood as merely a call to redefine or reclassify it, involving a taxonomic dispute of the sort familiar to historians. A scholar may deny, for example, that there was a Renaissance in the thirteenth century and mean by the denial only that conditions prevailing in the thirteenth century do not fit his or her definition and taxonomic requirements for a "genuine" Renaissance. This essay is not that kind of challenge to the balance of power interpretation of the Vienna settlement. It may seem so at times, simply because the case to be made here, necessarily a prima facie one, involves some definition and interpretation of balance language and ideas. It will always remain possible to argue that a different definition of balance of power would meet the objections to be raised in regard to 1815. Nonetheless, the thesis is not that the 1815 balance of power needs to be defined or understood differently, or that the settlement represented a particular, modified kind of balance of power,[3] but rather that any balance of power interpretation of the Vienna settlement is misleading and wrong. Its essential power relations were hegemonic, not balanced, and a hegemonic distribution of power, along with other factors, made the system work. A move away from eighteenth-century balance of power politics to a different kind of politics was an essential element in the revolutionary transformation of European international politics achieved in 1813–1815.

LET US TURN FIRST TO WHAT ACTUALLY HAPPENED in 1813–1815—whether power was distributed in a "balanced" way and if balanced, in what sense, and whether the system operated and sustained itself by a recognizable balance of power mechanism.

These questions call for a definition of the balance of power—a notoriously slippery, vague, and protean term, repeatedly debated and variously defined. One political scientist has identified nine different meanings of the phrase in the literature, including one in which "balance of power" means hegemony. I have found eleven different meanings of the term, some contradictory to others, in the working language of nineteenth-century diplomacy.[4] Moreover, even if the literal meaning or denotation of the phrase were clear, that would not end the difficulty. For what the term connotes and involves in the form of necessary conditions, component elements, and corollaries is also controversial. Balance of power analysts, for example, have long broken lances over whether or not a balance of power system presupposes or requires such things as an even distribution of power, the absence of dominant coalitions, the presence or permanent possibility

[3] This approach is quite common. Both Gulick's and Kissinger's works employ it, as does F. H. Hinsley, *Power and the Pursuit of Peace* (Cambridge, 1963); and Hedley Bull, *The Anarchical Society* (New York, 1977). Richard Little, "Deconstructing the Balance of Power: Two Traditions of Thought," *Review of International Studies*, 15 (1989): 87–100, contends that the concept of balance of power contains both an adversarial and an associative tradition, the 1815 settlement exemplifying the latter.

[4] E. B. Haas, "The Balance of Power: Prescription, Concept, or Propaganda," *World Politics*, 5 (1953): 442–77; P. W. Schroeder, "The Nineteenth Century System: Balance of Power or Political Equilibrium?" *Review of International Studies*, 15 (1989): 136–37.

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of blocking coalitions, flexibility in alliances, a holder or manager of the balance, and the existence of independent smaller states.[5]

Yet the confusion and flexibility attending the term do not preclude stating a reasonable minimum definition of it, identifying certain features and conditions as *sine qua non*.[6] A "balance of power system" must mean one in which the power possessed and exercised by states within the system is checked and balanced by the power of others. All the major actors in the system must be subject to this kind of restraint, at least potentially having to fear the countervailing power of a blocking coalition or other deterrent action by other states, should they upset the balance by aggression, threat, or an inordinate growth in capability. The requirements usually cited for a balance of power system—alliance flexibility, the existence of at least two or more actors of relatively equal power, the desire of all states to survive and maintain their independence, and the possibility of the use of force—are all corollaries of this basic requirement. The goals of balance of power, whether they be peace and stability or simply the preservation of the independence of the system's member states, are supposedly reached by maintaining this kind of countervailing balance.

This definition fits not only ordinary usage but also the way the Vienna system has been conceived. The allied statesmen, it is supposed, reduced France nearly to its ancient limits, restored Austria and Prussia to their size and status of 1805–1806, strengthened the states bordering France as a barrier against the renewal of French aggression, guaranteed the independence and integrity of all European states, especially the smaller ones, and united Germany in a defensive confederation, all in order to restore a balance of power that would deter any great power from endangering the integrity, essential interests, or independence of any other state, as France had done for a generation.[7]

The same power-balancing mechanism is also supposed to have worked after 1815 to ward off such threats to the balance as French revisionism, Russia's potential menace to the Ottoman empire and its aspirations to world leadership, Prussian ambitions in Germany, or Austrian and Prussian rivalry. The blocking alignments naturally changed with changing conditions. After the original Quadruple Alliance against France became obsolete and the initial Anglo-Austrian combination to check France and Russia broke down in the early 1820s, a liberal-constitutional camp emerged in the West in the 1820s and 1830s to counterbalance the Eastern Holy Alliance. British statesmen such as George Canning and Lord Palmerston, manipulating the balance and exploiting various rivalries within and between the blocs, successfully managed crises (the Greek revolt in the 1820s, the Belgian problem of 1830–1839, and the Near Eastern

[5] Morton Kaplan's attempt to define the working rules of a balance of power system in his *System and Process in International Politics* (New York, 1957) has been much discussed; see my "Nineteenth Century System," nn. 4–5, as well as the entire April 1989 issue of *Review of International Studies*, 15; also Bull, *Anarchical Society*, chap. 6; and Barry Buzan, *People, States and Fear* (Brighton, Sussex, 1983), esp. 101–03.

[6] Robert Jervis, "From Balance to Concert: A Study of International Security Cooperation," in Kenneth A. Oye, ed., *Cooperation under Anarchy* (Princeton, N.J., 1986), 60–61.

[7] In addition to the works cited above in n. 1, see J.-H. Pirenne, *La Sainte-Alliance, organisation européenne de la paix mondiale: Les traités de paix 1814–1815*, 2 vols. (Neuchâtel, 1946–49).

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crisis of 1839–1841) in ways that preserved peace, maintained the balance system, and promoted British interests.[8]

The balance of power interpretation of the 1815 settlement seems so obvious and coherent that certain basic factual questions are sometimes overlooked. Did the actual distribution of power in 1815 meet the minimal requirements for a working balance of power? Was power apportioned so that all the major actors and actual or potential power blocs were subject to countervailing power from others?

Here it is important to remember that power in international politics does not derive solely, or often mainly, from capability aggregation—growth in territory and population, resources, number of men under arms, industrial and technological development, fiscal and economic strength, and other factors affecting state power. Effective power is inextricably related to security, which in political terms is determined largely by the relationship between capabilities and vulnerability, the resources available to the state and the actual and potential demands on those resources in the international arena.[9] Eighteenth and early nineteenth-century statesmen knew this, of course, and calculated power along precisely these lines. The factors they normally used to measure power and to calculate gains and losses were often fairly crude—population, revenues, size of armed forces, and size of territory; but their evaluations of their own power and security and that of other states regularly involved comparing capabilities against vulnerabilities or threats.[10]

Even on the basis of raw power, resources, and capabilities, one can hardly speak of a balance of power in 1815. Prussia, for example, never achieved more than marginal status as a great power under Frederick II in the eighteenth century and had to strain to maintain that nominal rank while hoping someday to become a truly great power by means of expansion.[11] Having barely survived in the Napoleonic period, Prussia emerged in 1815 as even less a real, independent great power than in 1789. Its fifteen million people and limited resources in no way compared with those of Russia, the British empire, or France. Austria was somewhat better off than Prussia but not decisively so.

Yet the imbalance in the great powers' intrinsic capabilities in 1815 and after pales in comparison to the imbalance in terms of their capabilities versus their

[8] In addition to Pirenne, *La Sainte-Alliance*, some major works on this period, all fitting the balance of power paradigm in different ways, are G. de Bertier de Sauvigny, *Metternich et la France après le Congrès de Vienne*, 3 vols. (Paris, 1968–71), and his *Metternich* (Paris, 1986); C. K. Webster, *The Foreign Policy of Castlereagh, 1815–1822* (London, 1934), and his *Foreign Policy of Palmerston 1830–1841*, 2 vols. (London, 1951); Kenneth Bourne, *Palmerston: The Early Years, 1784–1841* (New York, 1982); Roger Bullen, *Palmerston, Guizot and the Collapse of the Entente Cordiale* (London, 1974); and H. W. V. Temperley, *The Foreign Policy of Canning, 1822–1827* (London, 1925).

[9] For a general discussion, see Buzan, *People, States and Fear*; for detailed analysis, Glenn H. Snyder and Paul Diesing, *Conflict among Nations* (Princeton, N.J., 1977).

[10] Frank A. J. Szabo, "Prince Kaunitz and the Balance of Power," *International History Review*, 1 (1979): 399–408.

[11] Wolfgang Stribrny, *Die Russland-Politik Friedrichs des Grossen 1764–1786* (Würzburg, 1966); Johannes Kunisch, *Das Mirakel des Hauses Brandenburg* (Berlin, 1978); Klaus Zernack, "Preussen—Frankreich—Polen: Revolution und Teilung," in Otto Büsch and Monika Neugebauer-Wölk, eds., *Preussen und die revolutionäre Herausforderung seit 1789* (Berlin, 1991), 21–42.

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vulnerability.[12] The so-called balance of power included five great powers, two on the flanks and three in the center. Each of the two flanking powers commanded more intrinsic resources than any of the other three. Russia had by far the largest territory, the greatest population, and the largest standing army; Britain dominated the seas while leading the world in industry, commerce, colonies, and financial power (indeed, if one takes India into account, as Edward Ingram and others have taught us we must, even in respect to land forces Britain was a great power).[13] At the same time, each of these flanking powers enjoyed virtual impregnability by virtue of geography, as Napoleon's failure to bring either of them down had proved. Geography also presented the three central powers, France, Austria, and Prussia—each of them markedly weaker than either of the flanking powers—with permanent, unavoidable strategic threats from their various neighbors. Thus Britain and Russia were so powerful and invulnerable that even a (highly unlikely) alliance of the three other powers against them would not seriously threaten the basic security of either, while such a (hypothetical) alliance would likewise not give France, Austria, and Prussia security comparable to that which Britain or Russia enjoyed on their own.

The system was just as imbalanced in regard to alliance capability, for the same reasons. Britain and Russia, not dependent on alliances for their basic security, could always make them when they wished and, from 1815 to 1848, regularly did. France, Austria, and Prussia, which were dependent on alliances for security, never gained alliances that made them truly secure militarily. (This is not to say that they lived in constant insecurity. All three of the major central states enjoyed more security from external threats in 1815–1848 than they ever enjoyed before or since. It is only to deny that this security derived from military alliances or a balance of power.) France constantly sought military allies after 1815 (mainly Britain, sometimes Russia), but its quest always ended in frustration. Even Austria and Prussia, closely allied and usually linked with one of the flanking powers, could not always count on their help. Britain, for example, was not available to meet a possible French attack on Austria in Italy in 1830–1832, and even Russian help was doubtful at that time, given the Polish insurrection. In any case, the German powers would always have had to bear the main brunt of a French attack themselves, as the Near Eastern crisis of 1840 illustrates.[14] Russia, to be sure, proved a loyal ally for both the German powers, especially after 1820, but this

[12] Various efforts to measure power and power relationships quantitatively undertaken by political scientists, including such distinguished scholars as Robert North, Richard Rosecrance, A. F. Organski, J. David Singer and Melvin Small, and others, have encountered grave conceptual and methodological problems—for example, how to quantify and weigh the impact of such factors as industry, natural resources, level of education, technological advancement, and national unity and morale on state power. William B. Moul, "Measuring the 'Balances of Power': A Look at Some Numbers," *Review of International Studies*, 15 (1989): 101–21, is both aware of the problems (Moul remarks, for example [p. 113], that "weighting each component equally is a *studied* confession of ignorance of the proper mixtures for each great power over a long period of time") and convincing as a rough approximation of capability ratios. What he shows is that two of the five great powers after 1815, Britain and Russia, always enjoyed a clear majority of the total capability (ranging from 55 percent to 70 percent) between 1815 and 1848. Even his study, moreover, cannot factor in their most decisive advantage, geographic invulnerability.

[13] Edward Ingram, *The Beginning of the Great Game in Asia, 1828–1834* (London, 1979).

[14] Irmline Veit-Brause, *Die deutsch-französische Krise von 1840* (Cologne, 1967).

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loyalty derived not from their military alliance or the balance of power but from their ideological solidarity in the Holy Alliance. Russia used its military alliances with Austria and Prussia under Catherine II (1762–1796) to keep both German powers insecure, dependent on Russia, and locked in rivalry with each other.[15] This was a normal balance of power policy; had Russia played a balance of power game after 1815, it would have done the same thing again, as its diplomats sometimes advised.

The disparities in power and security among the five powers in 1815 and after are great enough to challenge the notion that the "Directory of Europe" or "Concert of Europe" derived from a European pentarchy of great powers. Certainly, the Concert existed, and it exercised a real, powerful influence on international politics;[16] but the idea that it rested on any sort of comparable power status among the five members is very doubtful. The usual definition of a great power is a country that can rely primarily on its own resources for the defense of its interests and existence. Both flanking powers undoubtedly qualify under this definition; France probably does also, though less clearly. But Austria, although qualified in terms of size, population, and potential resources, suffered from so many external and internal weaknesses and vulnerabilities that it could not possibly meet the most likely external threats it faced (a war against France or a major revolution in Austria's sphere of influence or within Austria itself) without getting immediate help from allies such as Russia, Prussia, and the German Confederation. A number of incidents illustrate the point—the revolutions of 1820–1821, the war scares of 1830–1832, the crisis of 1840–1841.[17] These were not solely, or mainly, a matter of Austria's internal weaknesses and rigidity. Before and after 1815, the Habsburg Monarchy exemplified what John Herz, Robert Jervis, and others have discussed as the "security dilemma": the inability of a state to meet external threats by increasing its capability, because every such exertion serves only to arouse its opponents to like or greater exertions, thus increasing its insecurity.[18] As for Prussia, it had the most slender resources among the five great powers, by far the most exposed and vulnerable frontiers, and the most extensive military obligations; there was no serious threat that it could have met without major allied assistance. Thus the balance of power in 1815 consisted of a pentarchy composed of two superpowers, one authentic but vulnerable great power, one highly marginal and even more vulnerable great power, and one power called great by courtesy only.

Still more imbalance prevailed in an aspect of international politics important to Europe since the fifteenth century and vital since the seventeenth, repeatedly a central issue in great wars, including the recent revolutionary and Napoleonic

[15] Isabel de Madariaga, *Russia in the Reign of Catherine the Great* (New Haven, Conn., 1982); Dietrich Gerhard, *England und der Aufstieg Russlands* (Munich, 1933).

[16] See, for example, Carsten Holbraad, *The Concert of Europe: A Study in German and British International Theory 1815–1914* (London, 1970); Richard B. Elrod, "The Concert of Europe: A Fresh Look at an International System," *World Politics*, 28 (1976): 159–74.

[17] Austria's relative weakness and overcommitment are often emphasized as strains on the 1815 system, for example by F. Roy Bridge and Roger Bullen, *The Great Powers and the European States System 1815–1914* (London, 1980), chap. 2.

[18] John H. Herz, *International Politics in the Atomic Age* (New York, 1959); Robert Jervis, *Perception and Misperception in International Politics* (Princeton, N.J., 1976), chap. 3.

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ones: the world outside Europe. The year 1815 left the two flanking powers with expanded and strengthened world positions—Russia in the Caucasus and Persia, Central Asia, the Far East, and North America, and Britain all over the globe. They could pursue these interests and try to expand their influence in various spheres almost without regard to the actions or reactions of the other powers. Likewise, neither was yet seriously constrained by imperial competition or conflict with the other. Their main world interests hardly touched in 1815. France was left meanwhile in a position essentially like Spain, Portugal, or the Netherlands, with residual colonies surviving on British sufferance,[19] and the other two "great powers" had hardly begun to think about playing an active role in the world outside Europe.[20]

On the reverse side of the coin, Britain and Russia, while effectively reserving world politics for themselves, also retained a dominant influence in areas adjacent to Continental Europe, long important in international politics, and often contested among European states—the Mediterranean, North Africa, the Levant, the Balkans and the Ottoman empire, and the Baltic—even where other powers had interests in these areas as important as Britain's and Russia's or more so. France could not expand its influence or activity in the Mediterranean or North Africa, or Austria in the Balkans or the Adriatic, or Austria and France in the Levant, or Prussia in the Baltic, without at least tacit permission from Britain or Russia or both. Russia generally had more to say about the fate of Serbia, the Danubian Principalities, Greece, and the Ottoman empire after 1815 than did Austria, and it enjoyed more influence in Denmark, Sweden, and the Baltic than Prussia. The Levant, in which historically French influence and trade had predominated and to which the British were relative newcomers, was dominated after 1815 by Britain; it controlled the western Mediterranean from Gibraltar and Malta. For decades, the British were able to block French plans to construct the Suez Canal in Egypt. The balance of power established in 1815, in other words, enabled two great powers to say to the others, "Our world spheres of influence are strictly ours; yours are European and therefore must be shared with us."

These imbalances were no accident. Broadly speaking, they represented the goals Britain and Russia had pursued throughout the Napoleonic wars and were the fruits of their victory. Not balance of power but hegemony—who would enjoy it and what forms it would take—had always been the focus of the wars of 1787–1815. By concentrating on the issues disputed at Vienna and the problems that later arose out of the settlement (Germany, Italy, Poland, Belgium, revolution and counter-revolution, liberal constitutionalism and nationalism versus monarchial conservatism), historians have obscured what was uncontested in the outcome, British and Russian ascendancy, and the fact that this was what British

[19] For the gradual revival of French interest in colonies, though without any desire to compete seriously with Britain, see Dieter Braunstein, *Französische Kolonialpolitik 1830–1852* (Wiesbaden, 1983).

[20] Manfred Kossok, *Im Schatten der Heiligen Allianz* (Berlin, G.D.R., 1964), tries to show a growing German, especially Prussian, interest in Latin America but fails to convince. As for Austria, it is striking how little the government did to try to make itself even an Adriatic and Mediterranean commercial and naval power before 1848. See, for example, Ronald E. Coons, *Seamen, Statesmen, and Bureaucrats* (Wiesbaden, 1985); and Lawrence Sondhaus, *The Habsburg Empire and the Sea* (West Lafayette, Ind., 1989).

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and Russian statesmen intended to achieve. Both powers constantly proclaimed their aim as the restoration of a balance of power on the Continent, but this means nothing—or rather, shows that their respective definitions of the European balance of power were hegemonic, as they had always been. For British statesmen, the European balance meant a weakened France restrained by other Continental powers and barriers, giving Britain a free hand to intervene or not in European affairs as its interests required and freeing it from serious rivalry on the seas or overseas. The British concept of Britain as the necessary disinterested holder of the balance was a good way of rationalizing its hegemonic role and reconciling it with balance of power theory and propaganda.[21]

The Russian understanding of the European balance was equally hegemonic. The balance in Europe was something Russia should hold and control, not something that would check Russia. "Balance of power" meant Russian predominance in Poland and a vague Russian paramountcy in the Baltic and Near East, with Germany not united but confederated under joint Austro-Prussian leadership, and both German powers, especially Prussia, closely allied to Russia and dependent on Russian support against dangers from France in Germany and Italy. These post-1815 ideas were simply a new, conservative version of Russia's traditional glacis policy, involving territorial and political arrangements designed to restrain the other powers and give Russia security to the west and south as well as influence and prestige.

BRITAIN'S AND RUSSIA'S PURSUIT OF HEGEMONY under the guise of balance of power was thus normal and traditional, merely less obvious after 1815 than it had been in the late eighteenth century, when it had been pursued overtly, aggressively, and by the early 1790s apparently with entire success. Britain in 1792 was dominant in Western Europe and overseas, the Dutch Patriot Revolt having been crushed, French influence routed, the United Provinces and Prussia allied to London, and France left mired in revolution. This enabled Britain to do what it wanted in India and to force the Spanish to diplomatic surrender at Nootka Sound. Russia, having defeated the Ottoman empire in 1768–1774 and 1787–1792, dominated the Black Sea and was on the march toward the Mediterranean, had reasserted its control over Poland and played the leading role in its partitions, and could exploit the rivalry between Austria and Prussia in Germany and the new tensions between revolutionary France and its neighbors. To cap these British and Russian bids for hegemony under the guise of balance, most Britons and many Russians considered their two governments natural allies, destined to be joint arbiters of Europe.

The pattern of emerging Anglo-Russian hegemony was neatly illustrated in the Anglo-Russian Ochakov crisis of 1791. The confrontation came about because each had expanded its sphere of influence so far that they briefly collided, immediately in the Ottoman empire, less directly over Central and Eastern

[21] For an example of such rationalization, see Michael Sheehan, "The Place of the Balancer in Balance of Power Theory," *Review of International Studies*, 15 (1989): 123–34.

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Europe. Yet the crisis arose not from real rivalry but from an attempt by the British prime minister William Pitt to define the terms of their potential partnership in running Europe. Pitt, like most Britons, considered Russia Britain's natural ally but saw Russia getting out of hand and needing to be curbed into partnership. Catherine was not averse to partnership with Britain but only on her terms. Pitt started the confrontation; Catherine won it.[22]

Britain and Russia were not alone in saying "balance" while meaning "hegemony." Almost everyone did. Austria's idea of a balance in Germany meant Austrian control of the Reich and supremacy over Prussia; Prussia's idea of a German balance meant an Austro-Prussian stalemate in the Reich, Prussian superiority in North Germany, and overall Prussian equality with Austria. Even the most moderate eighteenth-century program for the balance of great powers, that of Louis XVI's foreign minister Count Vergennes, involved elements of French hegemony—and Vergennes' moderation was one reason why his program ended in disastrous failure.[23]

The pattern in which balance of power slogans and rules served hegemonic aims persisted through most of the wars of the French Revolution and Napoleon. Far from these being wars unleashed by French hegemony and imperialism and fought by the allies to restore a balance of power, they represented for at least the first fifteen years (1792–1807) a great contest between three bids for hegemony, British, Russian, and French, with the rest of Europe, small and former great powers—Prussia as well as Austria—trying to survive the contest and where possible profit from it. French statesmen naturally used balance of power arguments and slogans to justify their hegemonic aims just as the British and Russians did, probably with the same mixture of disingenuousness and sincerity, and the French arguments were at least as objectively valid as those of their opponents.[24]

Had France, before or during Napoleon's reign, ever been willing and able to consolidate its control of Western Europe (it had chances to do so, but the revolutionary governments did not pursue them seriously or consistently and Napoleon never considered them at all), a genuine system of balance of power could conceivably have arisen in Europe, with French power balancing the obvious geographic advantages of Britain and Russia. It would not have been a

[22] John Ehrman, *The Younger Pitt*, Volume 1: *The Years of Acclaim* (New York, 1969); Paul L. C. Webb, "Sea Power in the Ochakov Affair of 1791," *International History Review*, 2 (1980): 13–33.

[23] Orville T. Murphy, *Charles Gravier, Comte de Vergennes* (Albany, N.Y., 1982).

[24] The French argued, among other things, that France's expansion merely restored the European balance destroyed by the partitions of Poland and the destruction or weakening of France's other traditional allies and intermediary bodies in Europe (the Ottoman empire, the German empire, Sweden), and by Britain's colonial conquests, and that without some effective check on Britain's domination of the high seas and its ruthless use of its naval power in war there could be no real balance in Europe. The arguments were, of course, self-interested propaganda—but far more reasonable propaganda than the corresponding British and Russian ones. Leaving rhetoric and propaganda aside, it is impossible to conceive of any balance of power in Europe in the 1800s that did not recognize and accord to France the natural hegemony in Western Europe, that is, the Low Countries, western Germany, northern Italy, and Spain, that existing military, economic, political, and even cultural relationships accorded it. France repeatedly had this kind of leadership in its grasp under Napoleon; sensible Frenchmen (Talleyrand, Baron d'Hauterive, Joseph Fouché, the marquis de Caulaincourt, and others) urged Napoleon to accept and consolidate it; many Germans and other Europeans were willing or eager to go along with it; but Napoleon threw it away.

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stable or peaceful system, but it would have been in some real sense balanced in terms of power. Instead, Napoleon's boundless ambition and egotism compelled the allies to carry the war to the point of defeat for France and the reduction of its power. With that, Europe fortunately lost any chance of establishing a working balance of power and was left with the power imbalances of 1815.

It was on the basis of these imbalances that the system worked after 1815. Historians may talk all they wish about checks and balances, rival alliances and ideological camps in the West and East, and the interpenetration of alliances and alignments in certain areas like the Low Countries or the Near East. This cannot disguise or obscure the most decisive fact about the system: nothing prevented Britain and Russia, whenever they chose, from combining to impose their will on the rest of Europe, regardless of the feelings, the interests, and even, in certain instances, the independence and integrity of other members. This happened twice after 1815, in 1826–1829 over the Greek question and in 1839–1841 over the second Muhammad Ali crisis in the Near East. In both cases, the other great powers, though dismayed by this Anglo-Russian coalition, could not stop it or break it up. In 1827, France had felt impelled to join it, and Austria watched helplessly while the allied intervention in Greece led to exactly what Austria had feared and predicted, a Russo-Turkish war and the threatened destruction of the Ottoman empire.[25] In 1840, the French premier Adolphe Thiers, fearing political defeat, goaded by French nationalist feeling, and outraged that Britain (actually, Lord Palmerston) should choose Russia over France as a partner in the Near East and exclude France from the Concert, resorted to a threat of war in Europe. Significantly, Thiers could not threaten war against France's opponents in the current crisis, Britain and Russia, but only against the powers France could get at militarily, Austria, Prussia, and the German Confederation. The crisis came to a close when Louis-Philippe backed down and replaced Thiers; the episode ended only months later with a chastened France's readmission into the Concert.[26] These episodes represent as good evidence of the hegemonic pattern of post-1815 European politics as one could ask for. Even when Russia and Britain were not in alliance, no combination of other Continental states could coerce or control them; and, when they joined forces, no state or coalition could effectively resist them.

It may be argued that these circumstances only modify the character and working principle of the balance of power system after 1815, rather than

[25] See especially the account in vol. 3 of Bertier de Sauvigny, *Metternich et la France*; also Temperley, *Foreign Policy of Canning*; and the standard accounts of the Greek War of Independence, including C. W. Crawley, *The Question of Greek Independence* (Cambridge, 1930); Douglas Dakin, *The Greek Struggle for Independence, 1821–1833* (Berkeley, Calif., 1973); C. M. Woodhouse, *The Greek War of Independence* (London, 1952); and Richard Clogg, ed., *The Struggle for Greek Independence* (Hamden, Conn., 1973).

[26] Besides Webster and Bourne on Palmerston's policy (n. 8 above), see Charles-H. Pouthas, "La politique de Thiers pendant la crise orientale de 1840," *Revue historique*, 182 (1938): 72–96; Afaf Lufti al-Sayyid Marsot, *Egypt in the Reign of Muhammad Ali* (New York, 1984); François Charles-Roux, *Thiers et Mehemet-Ali* (Paris, 1951); and J. P. T. Bury and R. T. Combs, *Thiers, 1797–1877* (London, 1986). A good overall interpretation is in H. A. C. Collingham, *The July Monarchy* (London, 1988). On the Franco-German confrontation of 1840 and its serious long-range results, see Veit-Brause, *Deutsch-Französische Krise*; Raymond Poidevin and Heinz-Otto Sieburg, eds., *Aspects des rélations franco-allemandes 1830–1848* (Metz, 1978); Anna Owsinka, *La politique de France envers l'Allemagne à l'époque de la Monarchie de juillet 1830–1848* (Wroclaw, 1974); and Dieter Roghe, *Die französische Deutschland-Politik während der ersten zehn Jahre der Julimonarchie (1830–1840)* (Frankfurt, 1971).

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disprove its existence. The 1815 balance of power may simply have been bipolar rather than multipolar, like the world balance after 1945, with Britain and its partners balancing against Russia and its allies. This explanation will not do, either. It makes sense to speak of a bipolar balance of power when, as during the Cold War, there were two well-defined blocs with clear-cut power alliances checking and restraining one another. But Britain and Russia did not operate in this fashion in the pre-1848 era. No arms race or competition for power set them at odds; they did not even compete very much for influence or colonies. They were leaders but not leaders of clear-cut rival blocs. They sometimes worked in close partnership, as in 1826–1829 and 1839–1841. Usually, despite their ideological differences and the beginnings after 1828 of a perceived rivalry between them in the Middle East and Central Asia, they left each other alone in their respective spheres.[27] Britain stood by while Russia crushed the Polish Revolt in 1830–1831 and helped extinguish the Free City of Cracow in 1846. Russia did not try to get in Britain's way in the Low Countries or the Iberian Peninsula. Not until Britain, joined by France, attempted in 1853–1854 to unite all of Europe against Russia were Anglo-Russian relations truly put on a balance of power basis—which led to the Crimean War and wrecked the Vienna system.[28]

The shared British and Russian hegemony prevailing after 1815 is only the most obvious reason for insisting that the settlement was not based on a balance of power. It was also permeated with sub-hegemonies. The German "balance" involved an Austro-Prussian partnership that managed the German Confederation and dominated the other German states. Austria's hegemony in Italy was virtually unchecked. For most of the post-1815 period, France had three Continental great powers allied against it, and it never gained reliable allies against this potential hegemonic threat. For decades, Central and Eastern Europe were dominated by the so-called Holy Alliance of the Eastern Powers. All these aspects of the settlement, as prominent in it and important to it as the hegemony and invulnerability enjoyed by Russia and Britain, represent additional problems for the standard view.

One should not expect too much precision and consistency in the use of political terminology, of course, especially with a term like "balance of power." Nonetheless, to describe an international system like that of 1815, in which such great disparities existed between the major actors (to say nothing of the many lesser ones) in terms of raw power, alliance capability, security, influence, and opportunities and freedom for action, as one of a "balance of power," is to block understanding and scholarly progress rather than advance it.

[27] Besides Ingram, *Great Game in Asia*, see G. D. Clayton, *Britain and the Eastern Question* (London, 1971); and David Gillard, *The Struggle for Asia, 1828–1914* (London, 1977). For evidence of how Russia exploited British collaboration for its own ends in 1826–1829, see Loyal Cowles, "The Failure to Restrain Russia: Canning, Nesselrode, and the Greek Question, 1825–1827," *International History Review*, 12 (1990): 688–720; on the late 1830s, see Harold N. Ingle, *Nesselrode and the Russian Rapprochement with Britain, 1836–1843* (Berkeley, Calif., 1976).

[28] Paul W. Schroeder, *Austria, Great Britain, and the Crimean War* (Ithaca, N.Y., 1972); Winfried Baumgart, *Der Friede von Paris 1856* (Munich, 1972); and Norman Rich, *Why the Crimean War?* (Hanover, N.H., 1985).

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THIS ESSAY COULD STOP HERE, claiming to have shown that whatever the 1815 system might have been, it was not one of balance of power. But a purely negative case such as this is rarely convincing, first because it seems to leave some important concerns and arguments for the prevailing view unanswered, second because it does not indicate why the prevailing and allegedly misleading view came to seem so convincing, and finally because it offers no plausible alternative. In other words, one needs somehow to anticipate the obvious objections to this revisionist view and answer, at least provisionally, some pertinent questions about it.

There are, it would seem, at least three plausible reasons for continuing to interpret the settlement of 1815 as based on a balance of power, despite the evidence to the contrary visible in the actual distribution of power and the operation of the system. The first objection is, "How can one otherwise account for all the balance of power language of the era, the repeated testimony of European statesmen that the settlement needed to rest and operate on a balance of power, or explain all the bargaining and haggling they did over territories before, during, and even after the Congress in order to achieve a workable balance?" The second is, "How, except on the assumption that a working, stable balance of power existed, can one explain the success of the Vienna system, the fact that for several decades peace was preserved and most governments chose to accept and operate within the system, believing that their interests were at least minimally protected by it—in short, that Europe was in balance?" The third is, "How could a system based on Anglo-Russian hegemony and on various sub-hegemonies produce a peaceful, stable Europe between 1815 and 1848, when, as this essay itself argues, earlier bids for hegemony, especially Britain's and Russia's at the end of the eighteenth century and France's under Napoleon, destroyed the system and produced a generation of war from 1792 to 1815? How could hegemony after 1815 suddenly come to be a recipe for peace and stability?"

These are serious objections. To indicate the broad answers: First, the language of the Vienna era certainly demonstrates that the international system required and rested on political equilibrium—but not that there was a balance of power. The same point applies to the bargaining that went on: it was necessary for producing equilibrium, not balance of power. Second, the stable, peaceful political equilibrium Europe enjoyed from 1815 to 1848 rose not from a balance of power but from a mutual consensus on norms and rules, respect for law, and an overall balance among the various actors in terms of rights, security, status, claims, duties, and satisfactions rather than power. Third, the difference between the nascent or emergent Anglo-Russian hegemony in late eighteenth-century Europe that helped wreck the European system and the shared Anglo-Russian hegemony in Europe in 1815 and after that helped make the system work is the difference between a predatory and a benign hegemony. The same applies, *mutatis mutandis*, to other sub-hegemonies in the Vienna settlement.

These bare-bones assertions cannot be demonstrated here, but they can at least be fleshed out somewhat. Having tried at some length elsewhere to establish the first point, that the language of the Vienna era reflected a search for political

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equilibrium rather than balance of power,[29] I will only summarize it briefly here. Statesmen in the Vienna era used a variety of different words and phrases in referring to the European balance. The most common are generic terms: "equilibrium," "European equilibrium," and "political equilibrium." What these meant or referred to was often vague, and their denotation and connotation can only be determined, if at all, by the context. Careful study makes clear, however, that two distinct though inextricably intertwined core meanings are involved in balance language. The one is legal and moral rather than political. "European equilibrium" means a condition of international stability, peace, respect for rights and law, the preservation of order, the supervision of international affairs and legitimation of change through the European Concert. This core meaning is unmistakably indicated in the frequent direct association of "equilibrium" with peace, the status quo, the maintenance of treaties, the preservation of the honor and rights of individual states, and similar desirable conditions. A phrase such as "équilibre des droits" marks this kind of meaning clearly. The other core meaning is the political one of "balance of power" in the ordinary restricted sense—an even distribution of power and policies of checking dangerous uses or accumulations of power by means of countervailing power. Such phrases as "equilibrium of states," "equilibrium of powers (*puissances*)," and "balance" often have a balance of power meaning, though not necessarily so. Others such as "équilibre de pouvoir" or "des pouvoirs," "balance du pouvoir," "Gleichgewicht der Kräfte" (I cannot recall any instances of "balance de force" or "des forces") and related phrases such as "répartition des forces," "contrebalancer" or "jeter un poids dans la balance" indicate clearly enough a process of balancing forces or power. Phrases such as these, though encountered less frequently than the others in this era, prove that checking and balancing power was one element in the process of achieving an overall balance in the system. But they also indicate that the two things were not seen as identical, that a balance of power was implicitly recognized to be part of a larger whole. In other words, the idea of redistributing power among states so that one power would in particular instances be balanced against another power was only one limited aspect of a balanced political order as European statesmen conceived it and represented only one partial means for achieving it. Labeling the whole settlement a balance of power is unacceptably reductionist.

I am not here twisting the language and meaning of the time to fit a particular theory, as some might think. It is rather the proponents of the balance of power view, especially British and American ones, who do this, doubtless unwittingly, when they routinely render terms such as "équilibre européen" or "politisches Gleichgewicht" into English as "balance of power." This mistranslation requires one to devote considerable scholarly effort and persistent argument to convey the simple fact that when European statesmen said "political equilibrium," they *meant* political equilibrium, no more and no less—and that political equilibrium was both broader than balance of power and different from it.

An important instance of the use of balance concepts and language at the Congress of Vienna will illustrate the point. Prince Talleyrand argued that there

[29] Schroeder, "Nineteenth Century System."

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could be no European equilibrium unless the legitimacy of all thrones was recognized, in particular that of the king of Saxony. As usual in politics, Talleyrand's contention was freighted with hidden purposes and connected with French and Bourbon interests. But Talleyrand also presented an argument in which he genuinely believed, which many shared with him, and which fits much thinking and practice at the Congress. One element in his argument—that the European equilibrium demanded the preservation of the throne and territories of the king of Saxony—fits the ordinary balance of power paradigm. To give Prussia all or most of Saxony, he contended, would make Prussia too powerful in northern Germany, threatening Austria and undermining the balance in Germany as a whole. But the main elements in Talleyrand's argument go far beyond questions of power, territory, and population, stressing the broad psychological and political implications of the question and making clear that he was principally concerned in his defense of legitimacy with upholding the moral and legal bases of the European equilibrium. Despoiling the king of Saxony, he insisted, would render every other German and European prince insecure on his throne and would revive the old politics of aggrandizement, force, and deceit born of fear and insecurity that Europe was trying to end and replace with a better system. The Napoleonic wars, he maintained, had not only destroyed the balance of power by making France too powerful but had destroyed all equilibrium ("tout équilibre") by undermining all rights, rendering all titles to any kind of property insecure, and substituting everywhere "l'empire de force" for the rule of law. The European equilibrium therefore had to be based on a restoration of the rule of law, beginning with its foundation, the security and legitimacy of all thrones—a principle as vital for the rights and security of the peoples of Europe as of their rulers.[30]

No one, I think, can study this period without concluding that Talleyrand here reflected the convictions of the great majority of European statesmen at Vienna. Like him, they used equilibrist arguments one-sidedly and self-interestedly, trying to extract advantages and promote particular interests; but they also genuinely understood and believed this notion of "legitimacy" to lie at the heart of European equilibrium and to constitute its central requirement. In a crunch, they would rather compromise or yield on their particular demands than destroy the foundation of the new system: respect for rights and the rule of law. In other words, the words "European equilibrium" implied a variety of things at Vienna. But they meant "rule of law" more centrally than "balance of power," and it was the consensus that existed among European governments on the moral and legal nature and requirements of the European equilibrium far more than any agreement about the necessary distribution of power that made the innumerable delicate transactions and compromises of the Vienna settlement possible.

This general framework indicates the answer to the second half of the question, the connection between the apparently ethereal, idealistic concept of the European balance and the practical business transacted at Vienna—the hard bargain-

[30] For instances of Talleyrand's arguments, see Talleyrand, *Mémoires*, 2: 132–33, 226–27, 230–33, 236–41, 254–55, 294–95, 402–03, 452–53; Polovtsov, *Corréspondance*, 1: nos. 35 and 87. Guglielmo Ferrero, *The Reconstruction of Europe* (New York, 1941), stresses this aspect of Talleyrand's thought.

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ing, changes of boundaries and rulers, and transfers of territories and peoples that unmistakably added up to a redistribution of power. There is no contradiction; a legal and moral balance, an *équilibre des droits*, was a major aim in the bargaining and required prolonged, intense bargaining to be achieved. The very term "balance of rights" implies this; the Vienna statesmen knew, as sensible ones must, that most quarrels in international politics do not pit right against wrong but rights against other rights. Consider the phrase commonly used to describe the allies' overall aim: "un juste équilibre par une sage répartition des forces." The adjectives mean something. The equilibrium must be just, correct; the redistribution of forces wise, suiting not merely the disputants but also the broader needs of Europe as a whole. This was a clear break with the competitive eighteenth-century code for balance of power transactions, in which everyone sought all possible compensations and indemnities simply in order to keep up with or get ahead of its neighbor and rival.

In addition, the needs of Europe were understood more in legal, moral, and socio-political terms than in terms of simple power. The way in which this *répartition des forces* was applied to its most important and immediate target, France, illustrates the point. Everyone, including most Frenchmen once they faced defeat, agreed that French power had to be reduced to limits compatible with the independence and security of its neighbors in Europe. It should then have been easy for the allies to agree on this goal and solve the problem in political terms, imposing the necessary reduction in power on France by force. Yet nothing like this happened. The allies in 1813–1814 did not approach the central and apparently simple problem of reducing France to manageable size mainly on the basis of calculations of power, as they so often had in previous coalitions. They seriously divided over how far France's power had to be reduced, with most of them ready during much of the period to let France keep its so-called natural frontiers and Napoleon retain his throne, which would have left France still very powerful and capable of aggression. More important, the main aim of all the allies in dealing with the French problem was to persuade the French government and people to *agree* with their proposed solution and accept it as good for France and Europe alike, with as little coercion by war as possible. Not only did the allies constantly, from early 1813 until about two weeks before the final surrender, bargain with France for a negotiated peace but they always intended that France should if possible emerge from the war relatively strong, not weak; united, not partitioned; secure, not threatened; and thereby satisfied, not discontented and restless. The constant allied appeals to France to make peace on a basis that would leave France stronger and greater than it had ever been under its kings were not mere propaganda: the commitment was amply fulfilled in the actual peace. Thus, even with France (indeed, especially with France), the statesmen of Vienna understood that a European balance required at least as much assurance as deterrence;[31] that a *sage répartition des forces* was needed for moral, political, and psychological equilibrium rather than merely for balance of power.

[31] I owe this phrase to a political science colleague, Professor Edward Kolodziej.

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THE DETERMINATION TO PREVENT FRENCH POWER from threatening the independence or security of other states by setting up barriers to French expansion—an unavoidable concern given France's recent record—had few if any counterparts in the rest of the settlement. All the other great powers remained capable of menacing their small neighbors. Britain could threaten any state vulnerable by sea and was expected to dominate the Netherlands; Russia was in a position to bully Sweden and Turkey if it chose; Austria could do so with the smaller states in Italy and Austria and Prussia with the lesser German powers. Only a few Frenchmen or representatives of smaller powers suggested erecting balance of power restraints to prevent the victorious great powers from abusing their power, and even these suggestions were not serious. This does not mean that statesmen at Vienna were unaware of or indifferent to the danger that the great powers would threaten or destroy the independence of smaller states. On the contrary, statesmen at Vienna were remarkably sensitive to the problem of small-power independence and security. It merely means that the allies chose moral, legal, and political means rather than balance of power measures to maintain a balance in this vital respect.

The main reason for redistributing territories, peoples, and thrones in Europe in 1814–1815 was not really to balance power against other power but, except for France to some degree, to reconcile claims among different states, to fulfill promises, to balance conflicting demands and requirements, to meet specific felt needs, to protect the independence and security of all states, and thereby to achieve a general equilibrium in which all the members of the European family of states would share in certain balanced advantages and duties. The process involved adjudication, mediation, and reconciliation more than weighing power; the balance sought in redistributing forces was mainly a balance of rights, that is, a balance between conflicting claims based on various rights. The Vienna equilibrium represented ideally a balance between what each state needed or claimed to need in order to fulfill its proper role and function within the European family and what that family as a whole considered necessary and proper. The redistribution was intended to produce a genuinely equal and fair balance in legal rights. Every state was to have its rights and territories recognized and sanctioned internationally. Rank, status, and privileges were also to be balanced, distributed according to each state's role and duties in the international system. The *répartition des forces* at Vienna was designed to bring many other vital elements of international relations into balance, even and especially where power was not balanced and could not be.[32]

[32] The concept of equilibrium as residing in a harmonious European family of states was of course not invented in 1813–1815 but emerged gradually in the course of the wars. For some examples of how different statesmen and theorists developed it, see, for Prussia's Baron vom Stein, Franz Herre, *Freiherr vom Stein* (Cologne, 1973), 87, 262–66; and Gerhard Ritter, *Stein* (Berlin, 1936), 374–75. For Spaniards during Spain's War of Independence (1808–14), see Rainer Wohlfeil, *Spanien und die deutsche Erhebung, 1808–1814* (Wiesbaden, 1965), 70–72, 88–89. For Metternich, his *Nachgelassene Papiere*, 1: 70–71. For Friedrich von Gentz, see Alexander Hase, "Friedrich (v.) Gentz: Vom Übertritt nach Wien, bis zu den 'Fragmenten des Gleichgewichts' (1802–1806)," *Historische Zeitschrift*, 211 (1970): 589–615. Immanuel Kant's doctrine of equilibrium in his *Idea of a Universal History from a Cosmopolitan Standpoint*, like that of Gentz, his pupil, was basically prescriptive and moral rather than political in character. See Lewis W. Beck, *On History* (New York, 1963), 11–26.

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The second question concerned how the Vienna system could have preserved peace so long and well without the restraining and deterrent effects of the balance of power—the knowledge that any state attempting aggression or a violation of the treaties would provoke an overwhelming coalition against itself. This plus the general post-1815 mood of war weariness and satiation, monarchial solidarity, and fear of revolution are supposed to account for the general peace after 1815.

Far too many axioms and assumptions are hidden in this apparently innocuous explanation of the post-1815 peace and stability to discuss in this essay. There is, for example, the assumption or implication that war weariness in 1815 accounts for the durability of peace but without an explanation of why a similar or greater war weariness after other great world wars (1648, 1713–1714, 1763, 1919, 1945) failed to have that salutary effect. Something of the same could be said for the notion that ideological solidarity and fear of revolution produced a peaceful international system. Why here and not elsewhere? There is the apparent question-begging assumption that successful deterrence of aggression and treaty violations must be due to a balance of power—as if deterrence is not a very tricky and unpredictable process, often serving to provoke aggressive behavior and produce spiraling conflict rather than curb or prevent it.[33] There is, finally, the tacit assumption that states fear isolation or confrontation by a hostile coalition in international politics essentially for reasons of power politics and security, when it clearly can be as much or more a matter of prestige or concern for the state's or the regime's honor and reputation.

At the heart of this objection, however, is the belief that aggression and unlawful behavior under the Vienna settlement were successfully deterred by balance of power means, by potential or actual threats of a blocking coalition, and this notion can be checked against the facts. Part of the answer has already been given. The two strongest states in the system, Britain and Russia, never faced even the threat of a hostile coalition to deter them by force from taking action. Nothing like this was ever contemplated against Britain, and the means proposed on various occasions to constrain Russia in the Near East involved diplomatic combinations designed to persuade it in friendly fashion to coordinate its policy toward the Ottoman empire with the other powers. Under the Vienna system, deterrence of Britain or Russia by balance of power means was not possible, not attempted, and, as things turned out, not necessary. As for the other so-called great powers, they could be deterred by either of the superpowers, France by Britain, Austria and Prussia by Russia, merely by the withdrawal of support rather than the application of pressure. A blocking coalition was never necessary and therefore never tried. Even France in 1840 was not really an exception to this rule.

Yet one should not conclude that the Vienna settlement offered no effective means of deterrence—quite the contrary. Deterrence under the Vienna system took the form of moral and legal political pressure, the threat that reckless or unlawful behavior would cost the offending state its status and voice within the

[33] See, for example, Paul C. Stern, *et al.*, eds., *Perspectives on Deterrence* (New York, 1989); Robert Jervis, Richard Ned Lebow, and Janice Gross Stein, eds., *Psychology and Deterrence* (Baltimore, Md., 1985); and Richard Ned Lebow and Janice Gross Stein, "When Does Deterrence Succeed and How Do We Know?" Occasional Papers, Canadian Institute for International Peace and Security, no. 8 (February 1990).

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system, leading to its isolation from it and the attendant loss of systemic rewards and benefits. This kind of deterrence was highly effective. It helped keep Russia from going to war with the Ottoman empire in 1821–1823, France from trying to exploit the Belgian revolution in 1830–1832, Austria and Italy from clashing over Italy, France from saber-rattling its way into war in 1840, and everyone from generally upsetting the peace.

Another point is not strictly related to the balance of power argument but is worth adding here. It is commonly supposed that the ruling monarchs and elites in 1815 accepted the compromises necessary to a stable peace mainly in order to avoid internal revolution. They needed international peace and solidarity to help them preserve the existing socio-political order and their privileged positions within it; to manage and, where necessary, to suppress the popular discontents created by war, postwar depression, and the spread of the ideas of the French Revolution and the popular liberal and national demands raised in the course of the War of Liberation. This view contains a kernel of truth but much more distortion. Of course rulers everywhere longed for peace, quiet, and a return to normalcy; so did their peoples. But just as it is incorrect to believe that the balance of power compelled governments to follow peaceful policies in 1815 and after, so it is wrong to suppose that the fear of revolution or of the popular pressures created by French revolutionary ideas or Napoleonic reforms motivated them to construct a more peaceful international system. This mistakes the reigning spirit of the era and stands the thinking and priorities of Europe's leaders on their heads. No major European government was ever threatened with overthrow by the ideas, propaganda, and subversion of the French Revolution, nor were many minor ones. What did destroy many minor thrones and sent those in Berlin, Vienna, and St. Petersburg reeling was war—not merely military defeat and the loss of territory and wealth but the stripping of their resources, the destruction of their armies, the danger that their soldiers, public servants, and peoples would rebel simply in order to have peace. The princes of Europe had little to fear from popular uprisings, certainly not from any inspired by the French Revolution. All the actual uprisings in Europe from 1798 through 1814, in Belgium, Switzerland, Sicily and Calabria, Spain, the Tyrol, Russia, and Germany, were traditional ones, mainly peasant in character and led by local notables and religious leaders, directed against the foreigner and driven by traditional loyalties to God, king, and country. The pressures for reforms and constitutions in 1815 and after came by and large not from the masses of people but from bureaucrats, government ministers, some members of the middle classes, and occasionally from centralizing princes themselves. Other segments of the elites opposed them. What the peoples overwhelmingly wanted in 1815 was much the same as what rulers wanted—an end to the strains, taxes, conscription, and sufferings of war. In short, European leaders were not driven to seek lasting peace in 1815 because they feared revolution. They did so because they feared war, had learned that war *was* revolution, and wanted no more of it.

Thus, however much the bargaining at Vienna may look like a revival of the old competitive eighteenth-century balance of power politics, it was different. A continuation of the process by which the final coalition of 1813–1814 had been

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built, the Vienna settlement involved achieving a consensus more than competing or fighting. The disputes were sometimes prolonged and difficult to settle precisely because typical eighteenth-century devices—the use of force and the violation of other states' rights and independence—had been ruled out, and only negotiation and compromise were left. The territorial quarrels between Austria and Bavaria in 1814–1816, for example, lasted as long as they did simply because Metternich would not and could not use the methods that Joseph II or Baron Thugut had used with Bavaria and other small German states before him (or, for that matter, the tactics that Count Montgelas, the Bavarian premier, had used under Napoleon against Austria). And, while most of the settlements and treaties of previous decades, especially those during Napoleon's reign, were never expected to last, those reached at Vienna were supposed to be permanent and therefore had to be negotiated with greater care.

As for the question of how hegemony, a factor in undermining the international system before 1815, became a stabilizing element then and thereafter, the answer is easy: the character and spirit of post-1815 hegemonies, both the main ones and the subordinate ones, were drastically different—selfish and predatory before, relatively benign, inactive, and tolerable thereafter. The transformation is most dramatic in the case of Russia. If one surveys Russian policy toward the Ottoman empire, Austria and Prussia, and the German empire during Catherine's reign and to some extent under Paul I and Alexander I until about 1812, and then compares it with Russian policy in the Vienna era, it is hard to believe the two policies belong to the same country. Pre-Vienna Russia was bent on destroying the Ottoman empire or at least gobbling up large tracts of its territory and exploiting its internal problems and weaknesses; post-Vienna Russia was bent on saving the Ottoman empire from collapse. Pre-Vienna Russia encouraged and exploited the rivalry between Austria and Prussia to paralyze them both and promote Russian ascendancy over Central Europe and Russian influence in the German empire; post-Vienna Russia encouraged a close partnership between Austria and Prussia, allowed them to manage the German Confederation on their own, and repeatedly told smaller German states pleading for Russian protection that they had to look to Austria and Prussia.

The change is only somewhat less startling in Britain. The British government before the wars and during much of their course cold-bloodedly exploited the weaknesses of its rivals France and Spain in order to expand its empire, especially in India and the West Indies but also in South Africa, the Indian Ocean, the South Seas, and Latin America. After the wars, Britain restored some of its colonial conquests, allowed its former rivals to maintain their empires and practice their mercantilist policies, and exercised its control of the seas mainly in a mutually beneficial policing fashion (against piracy and the slave trade). France before 1815 was Britain's natural and necessary enemy, to be reduced and constrained as much as possible. France after 1815, though still an object of suspicion, was Britain's normal partner in European affairs, to be restrained through watchful partnership. Britain under William Pitt and William Grenville in the 1790s treated the United Provinces as a British satellite that was supposed to follow British policy in Europe and pay for British support with commercial and colonial

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concessions. Britain under the earl of Liverpool and Viscount Castlereagh in 1813 and after not only helped create an expanded Kingdom of the United Netherlands but allowed it to pursue an independent role in Europe and a mercantilist colonial and commercial policy in the Far East.

Similar turnarounds took place in the sub-hegemonies in Europe. The contrast between Austro-Prussian rivalry in Germany and exploitation of the Reich estates before 1800 and Austro-Prussian cooperation in creating and managing the Confederation thereafter is dramatic. True, their leadership, especially Austria's, was politically repressive after 1819, but Prussia's creation and leadership of the German Zollverein is a perfect example of benign hegemony.[34] The main reason Metternich could turn the Confederation into an instrument of political standstill is that most governments feared revolution and wanted Austria's support against it. Austrian hegemony in Italy was too exclusive and politically conservative, but it was legal, unaggressive, and non-expansionist, in stark contrast to Austria's Italian ambitions during much of the war. On the whole, Austrian support was welcomed by Italian regimes as a barrier against revolution and war. In the one instance in which a post-1815 hegemony cannot be termed benign, Poland, we can still cite a failed attempt to create one—the failure of the Congress effectively to guarantee the special position of the Polish territories within the three partitioning powers, and the failure of these three powers' measures, especially Russia's, to satisfy the Poles with cultural and administrative autonomy.

One more aspect of the hegemonic Vienna system must be mentioned: the fact that while Britain and Russia each exercised a certain hegemony in their respective spheres, they did not usually combine their forces to dominate Europe, nor did they try to exploit the states between them in a rivalry with the other. Thus the twin nightmares of Continental Europe—Anglo-Russian intimacy and Anglo-Russian enmity—both of which had menaced and oppressed Europeans in the pre-Vienna era, were avoided. Eighteenth-century balance of power rules and practices produced predatory, destabilizing hegemony. The Vienna era's equilibrist rules and practices promoted benign, stabilizing kinds of hegemony.

WHAT ABOUT THE POLISH-SAXON CRISIS at the Congress of Vienna? It certainly looks like a classic balance of power confrontation—Russia insisting on taking the lion's share of Poland, thereby threatening Austria, Prussia, and Germany; Russia's partner Prussia demanding all of Saxony; Austria, France, and Britain resisting in the name of balance of power; Russia and Prussia threatening the use of force; the other three powers concluding a secret defensive alliance on January 3, 1815; and, finally, after a crisis and threat of war, Russia and Prussia accepting a compromise. Appearances, however, are deceptive. The case is far too complex to discuss thoroughly here, but a brief mention of the high points suffices to show that the Polish-Saxon crisis does not represent a case in which balancing tactics preserved a balance of power in Europe. It was instead the occasion when the old eighteenth-century balance of power politics flared up most dangerously at

[34] See especially Hans-Werner Hahn, *Die Geschichte des Deutschen Zollvereins* (Göttingen, 1984).

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Vienna but failed in the end to prevail. The balance of power contest ended with a hands-down victory for the hegemonic power threatening the balance, Russia. But, after winning, Russia shrank back from the cost of its victory in terms of the general European balance and helped promote a compromise that, far from establishing a new balance of power, confirmed and strengthened both Anglo-Russian hegemony and European political equilibrium.

A few basic points drawn from any standard account will help make this interpretation at least plausible.[35] First, for Austria, the state most threatened by the Russo-Prussian plans for Poland and Saxony, the critical issue was not balance of power but political equilibrium. Metternich always knew, and made clear to Russia, that while Austria disliked and feared the great expansion of Russia militarily and territorially in Poland, it could live with it. What Austria could not accept was Alexander I's intent to create a constitutional, autonomous Kingdom of Poland out of Russia's share, raising a permanent revolutionary threat and making both Austria's and Prussia's Polish territories ungovernable.[36] Thus the contest was more about political equilibrium, the terms on which the Eastern powers would cooperate in governing their respective Polish populations, than balance of power. On this score, Austria simply had to accept defeat. Alexander went ahead with his constitutional plans for Poland and, as Metternich predicted, thereby helped create a revolutionary problem that recoiled on Russia more than on Austria and Prussia.

Second, Austria, it is true, at first attempted to meet the Russian threat by balance of power tactics, notably the Metternich-Hardenberg agreement of October 22, 1814, which granted to Prussia all of Saxony if it would join Austria in a strong anti-Russian stand over Poland. But this strategy collapsed within two weeks when Alexander pressured Prussian King Frederick William III into denouncing the Metternich-Hardenberg agreement and lining up solidly behind Russia. Alexander also successfully defied all the balance of power arguments and pressure of Castlereagh. By late November, Metternich accepted his defeat and sought a rapprochement with Russia to prevent both Russia and Prussia from winning at Austria's expense. Thus Russia easily won the balance of power contest in the Polish-Saxon affair by early December 1814. Moreover, Russia never surrendered this victory or compromised it away. Alexander went ahead with his Kingdom of Poland; in the final settlement, Russia retained most of the territory it had earlier claimed; and Russian Poland remained territorially and strategically a threat to Austria, Prussia, and the later German Reich until World War I.

Third, the real crisis and threat of war in Europe arose after Russia's balance of power victory, because of its effects on Europe's emerging and still fragile political equilibrium. Austria, accepting defeat by Russia, now withdrew its promise to let Prussia have Saxony and abandoned its cooperation with Prussia in a general

[35] The best recent one is in Kraehe, *Metternich's German Policy*, chaps. 8–10; but see also Ulrike Eich, *Russland und Europa: Studien zur russischen Deutschland-Politik in der Zeit des Wiener Kongresses* (Cologne, 1986).

[36] August Fournier, "Zur Geschichte der polnischen Frage 1814 und 1815," *Mitteilungen des Instituts für Österreichische Geschichtsforschung*, 20 (1899): 444–75; Fournier, "Londoner Präludien zum Wiener Kongress," *Deutsche Revue*, 43 (1918), 1: 125–36, 205–19; 2: 24–33; Nesselrode to Alexander I, September 25, 1814, *Vneshniaia politika Rossii*, ser. 1, vol. 8, no. 46.

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German settlement. Prussia, facing heavy losses, threatened to annex Saxony with or without the consent of the other powers, and this rash threat created some danger of war. Alexander, dismayed at the side effects of his victory (he had driven Britain and France together into opposition, made Austria defect to the West, put Prussia in serious danger, promoted a breakdown of the German settlement, and put the entire European alliance he aspired to lead at enormous risk), intervened to save the situation, not so much by backing down himself but by compelling his own junior partner Prussia to accept major sacrifices in Saxony. While the tsar forced Prussia to pay the main costs of a compromise, sweetening the pill with some concessions to Prussia in Poland, Castlereagh kept France and Austria in line through the Triple Alliance of January 1815 and helped reconcile Prussia to defeat through concessions in Saxony and the Rhineland. As Castlereagh's and Liverpool's correspondence makes clear, the Triple Alliance, which was directed against Prussia rather than Russia and only concluded after the worst crisis was over, was never intended for purposes of war. Liverpool was convinced that the British public and Parliament would not stand for it, and he approved of the alliance as a way of avoiding war and particularly of controlling Austria.[37]

In other words, balance of power politics was tried in the Polish-Saxon crisis, and it led, as it usually does, to a victory for the hegemonic power. When the participants saw what balance of power politics was leading to, they backed off and, under the lead of the hegemonic powers, returned to the practices of political equilibrium.

THIS ESSAY HAS COVERED MUCH GROUND already, inevitably hastily and superficially, and it may be foolish to break more. Critics will surely find enough to object to. Yet one more question is bound to occur even to a receptive reader: If the case against the balance of power interpretation is so clear, why have many excellent scholars adopted it? The question need not be answered to sustain the case, but it is a natural and legitimate one, and there are possible answers.

Among the factors that would seem to help account for the persistence of the balance of power paradigm are the extraordinary flexibility and protean character of the concept (not even democracy, socialism, or Christianity match it in ability to be all things to all people), the weight of tradition and conventional usage, and especially the hold that so-called realist notions about international politics have over scholars in the field, political scientists and historians alike. It seems axiomatic to many persons that, although rules, norms, and mutually accepted practices can modify and influence the conduct of international affairs to some extent, ultimately international politics must rest on power and reduce to relations of power. Thus they find a fundamental challenge to this notion, a denial of the sway of balance of power in a particular era, hard to take seriously.[38]

[37] Webster, *British Diplomacy*, 266–67, 277–80, 284–85.

[38] Even a holistic critic of Kenneth N. Waltz and his structural realism (*Theory of International Politics* [New York, 1979]) such as Barry Buzan (*People, States and Fear*, 102–03), endorses Waltz's fundamental premise that balance of power is by definition a necessary result and reflection of state sovereignty and the resultant structural anarchy of international politics.

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To this general observation can be added a specific one: the persistence of the balance of power interpretation of the Vienna settlement is part of a larger historiographical problem—the failure or refusal of historians to recognize this settlement's truly revolutionary character, its uniqueness among all the peace settlements of European history. Time and again, leading scholars concede its virtues of moderation and durability, frequently noting at the same time that these virtues were purchased at a high price in terms of liberty, reform, and progress, and then pass on. The Vienna settlement is thus assigned a normal place in the series of international settlements, all of which are supposed to have worked for a time because of war weariness and temporary ideological solidarity but sooner or later to have decayed and broken down. In this way, historians are free to concentrate on the really important revolutions and sources of change in the early nineteenth century—domestic-political, social, economic, intellectual, and cultural. But the most obvious fact about the Vienna settlement, plain to everyone willing to look at it, is elided and obscured: 1815 is the one and only time in European history when statesmen sat down to construct a peaceful international system after a great war and succeeded; the only settlement, unlike 1648, 1713–1714, 1763, 1919, 1945, and many others, that was not accompanied or quickly followed by renewed or continued conflict, revived tensions, arms races, and competitive balance of power politics. This astonishing accomplishment in international politics, moreover, this uniquely non-utopian revolution, made possible much of the structural change and progress in nineteenth-century European society. It is high time that this extraordinary accomplishment be squarely recognized, high time that an answer be given those who insist that international politics never really changes, that it always must remain in the old cycles of balance of power and systemic breakdown and conflict, high time to point to the Vienna era and say with Galileo, "Eppur, si muove" (and yet, it moves).

The alternative interpretation of the Vienna system offered here, that it rested not on balance of power but on hegemony, also has implications for historians of international politics. It may be time to give the concept of balance of power a rest. The term is doubtless unavoidable and can serve useful purposes, but it has been so abused and become so misleading that it would benefit from a period on the shelf. Spend the time instead on hegemony—its importance as an organizing principle in international politics, its legitimate and fruitful uses as well as its dangers, the possibilities of peace through the right organization and use of hegemonies, and the dangers of attempting to erect artificial balances of power in situations of natural hegemony. There is plenty of political science theory to apply, if one wants it: hegemony theory, regime theory, cognitive theory, deterrence theory.[39] There are plenty of eras besides that of Vienna to which to apply it. In discussions of the origins of World War I, for example, compare the amount of attention paid to Imperial Germany's growth in power and its

[39] For example, Robert Gilpin, *The Political Economy of International Relations* (Princeton, N.J., 1987); and *War and Change in World Politics* (New York, 1981); Robert O. Keohane, *After Hegemony: Cooperation and Discord in the World Political Economy* (Princeton, 1984); Oye, *Cooperation under Anarchy*; Jervis, *Perception and Misperception*.

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supposed threat to the balance of power with that paid to the decline or disappearance of British and Russian hegemony, phenomena at least as obvious, far more easily demonstrable, and at least as clearly connected with the breakdown of the system. Paying more attention to hegemony (a neutral word, after all, simply denoting a factual condition of leadership or primacy, without the inescapable pejorative connotations of colonialism or imperialism) might not only be profitable for the study of history but have useful implications for the international world of the late twentieth century.

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AHR Forum

A Bipolar Balance of Power

ENNO E. KRAEHE

IN THE BOLD AND INCISIVE LANGUAGE that we have all come to admire in his work, Paul Schroeder insists he is not merely tinkering with the definition of the term balance of power but discarding the concept altogether, at least as an interpretation of the Vienna settlement. The settlement's "essential power relations were hegemonic, not balanced," he says, and further, this is what made the system work. In this respect, he adds, the Vienna settlement of 1815 differed from all other settlements in modern times, most particularly the predatory balance of power politics practiced in the eighteenth century.[1]

Before challenging this thesis, as this rejoinder is meant to do, it is important to acknowledge broad areas of agreement with the picture Schroeder paints of hegemonic politics. First of all, his critique of the conventional explanations for relative stability is essentially sound though not entirely free of inconsistencies. War weariness there certainly was but unevenly distributed. Britain was physically unscathed and financially sound; Russia in 1815 was just reaching the peak of its military build-up, which in turn depended heavily on British subsidies and foreign enlistments; Prussia actually favored war over Saxony; Austria was bankrupt, but that was its normal condition. Nor does war weariness explain why peace reigned after 1815 but not long after many other debilitating conflicts such as the War of the Austrian Succession.

Schroeder likewise doubts that a common fear of revolution drove fearful monarchs to huddle together in a common front against their rebellious peoples. This version of the Concert, parading the Holy Alliance and the Troppau Protocol as its prime exhibits, became liberal, and nationalist orthodoxy until well into this century; and until just recently it continued to reign in the countries of the former eastern bloc, representing the Congress of Vienna as a ruling-class conspiracy, a classic demonstration of a class struggle across international boundaries.[2] Schroeder's refutation is valid; in making it, however, he comes close to crediting war weariness after all. "European leaders," he writes, "were not driven to seek lasting peace in 1815 because they feared revolution. They did so because

[1] Paul W. Schroeder, "Did the Vienna Settlement Rest on a Balance of Power?" *AHR*, 97 (June 1992): 684, 702, 705.

[2] See, for instance, Karl Obermann, "Der Wiener Kongress 1814/1815," *Zeitschrift für Geschichtswissenschaft*, 13 (1965): 474–92. See Andreas Dorpalen, "The German Struggle against Napoleon: The East German View," *Journal of Modern History*, 41 (1969): 485–516, for other examples.

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they feared war." Technically, fear of war may not be quite the same as war weariness, but in this context, where it is advanced to explain the peculiar quest for consensus at Vienna and the grounds for "ruling out the use of force," we face a muddle that deserves more explanation than provided here.[3] As first presented, however, the downgrading of fear of both war and revolution is well taken, and the notions ought to be retired from our textbooks.

A more significant area of agreement is Schroeder's description of the power relationships established in 1815 and consolidated in the next eight years or so; it is essentially correct, a considerable improvement over restoration, legitimacy, compensation, and similar clichés often used to describe the settlement. In contrast to the greater measure of equality among the powers during the eighteenth century, the Vienna settlement clearly marked the emergence of two superpowers. Britain and Russia were not only immensely stronger than any of the others but were also invulnerable to each other. Each exercised a hegemony in a sphere that it was capable of defending: Britain on the high seas and in the colonial world, Russia in Eastern Europe and much of northern Asia. The best that the powers in between, the middle powers as they are now frequently called, could hope for was to stake out sub-hegemonies of their own. This they indeed tried to do: France in Western Europe, Austria in Central Europe, and Prussia in northern Germany. One could go further even than Schroeder does to point out that, within these spheres, each deliberately rejected regional balances in favor of hegemonic solutions, although with varying degrees of success. Instead of the nice balance between Austria and Prussia that Talleyrand had hoped for in Germany, Metternich strove for Austrian dominance through the German Confederation and by 1820 achieved it—over Russian opposition even more than Prussian, it should be noted.[4] Similarly in Italy, a three-power balance among Austria, France, and Britain was a possibility, but that was not enough; Metternich wanted Austrian hegemony and in 1820 was ready to fight for it. The "Comte de Balance," as he was rightly called at the Congress of Vienna for his exertions for a European equilibrium, thus had his limits; in fact, self-serving though it may have been, his premise was that the Continental equilibrium *required* Austrian preponderance in the center.

Prussian and French aspirations to regional hegemony fared less well. From 1815 to 1820, Prussia strove to impose its military control over northern Germany but was no more successful in this endeavor than it had been earlier in the political realm. France did better, though gradually, in stages. In 1818, with generous Russian support, France regained control of its own soil. In 1823, France conquered Spain and laid the groundwork for a possible three-power challenge to Britain's position in Latin America. Though shared to some extent with England in the 1830s, French influence in Madrid was paramount with few interruptions until the Hohenzollern candidature in 1870. The neutralization of Belgium and the razing of the barrier fortresses in the 1830s were further French gains, and

[3] Schroeder, "Did the Vienna Settlement Rest," 700, 701.

[4] Enno E. Kraehe, "Austria, Russia and the German Confederation 1813–1820," *Deutscher Bund und deutsche Frage 1815–1866*, Helmut Rumpler, ed. (Vienna, 1990), 264–80; *Wiener Beiträge für Geschichte der Neuzeit*, 16–17 (1989–90): 264–80.

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even though they did not constitute hegemony, France's position vis-à-vis the low countries was henceforth stronger than it had been in most of the eighteenth century, even if its overall position in the world, as Schroeder reminds us, was a far cry from former glories.

One important exception to the establishment of regional hegemonies was the territory covered by the Ottoman empire. Here the great-power groping to determine a regional winner had no conclusive or lasting outcome, which is why it became the primary theater of confrontation down to Sarajevo. To sum up, although I would prefer the term "spheres of influence" to hegemony/sub-hegemony to describe the outcome of the Vienna settlement, I have no objection to the pattern of relationships Schroeder means to convey. We shall return to this issue.

IN THE MEANTIME, there are several other terms that require definition. One is balance of power itself, and here again Schroeder's minimal definition is perfectly serviceable for present purposes: a system "in which the power possessed and exercised by states . . . is checked and balanced by the power of others."[5] What does it mean, however, to ask if the Vienna settlement (or any other) *rested on* such a system? Are we talking merely about a de facto condition resulting when the powers, each a predator, each pursuing its selfish interests, fortuitously find themselves checked one by the other? (Recall the words of George Canning, who exultantly called the breakdown after the Congress of Verona "a wholesome state: every nation for itself and God for all!"[6]) Or do we mean that the Vienna settlement represented an intelligent design deliberately crafted by a generation of statesmen steeped in the doctrine or philosophy of balance of power—"equilibrists" as Edward Vose Gulick called them?[7] The latter, reinforced by various instruments of international cooperation such as Quadruple Alliance, Holy Alliance, Concert, Pentarchy, is the definition most conspicuous in the literature, and Schroeder is right to reject it, arguing that in reality the so-called equilibrists were pursuing hegemony, however much they preached balance.

Nevertheless, the author is obviously not denying any of the benign qualities that the model imputes to doctrinaire equilibrists. On the contrary, his central point is that their restraint and conciliatory ways were real but derived from other premises: namely, a concern for the rule of law, with its foundations in the security and legitimacy of all thrones, "a principle as vital for the rights and security of the peoples of Europe as of their rulers." European equilibrium, he proclaims, meant "'rule of law' more centrally than 'balance of power.'"[8] At this point, our patient acquiescence runs out; it is time to mutiny.

[5] Schroeder, "Did the Vienna Settlement Rest," 685.

[6] Cited by Irby Nichols, *The European Pentarchy and the Congress of Verona 1822* (The Hague, 1971), 315.

[7] *Europe's Classical Balance of Power* (Ithaca, N.Y., 1955). Gulick's equilibrists are strikingly different from the one in the dictionary definition: "one who balances himself in unnatural positions and hazardous movements as in rope dancing."

[8] Schroeder, "Did the Vienna Settlement Rest," 696.

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In making these judgments, Schroeder writes like a jurist rather than a chronicler of the real world. It is easy to find dozens of expressions of such idealism, as it always is in human affairs; but to document the cogency and relevancy of such principles in concrete cases is more difficult, often enough is merely conjured out of thin air. His language is abstract as in speaking of "the allies" doing this or desiring that, as if they constituted a collective entity with a common will, a common purpose, a common philosophy.

Take his explanation for the relatively gentle treatment of defeated France. From the beginning, he maintains, all the allies wished to keep France reasonably strong. Instead of imposing a solution, they negotiated (the standard procedure in the eighteenth century, as it happens). True, but largely because Metternich, threatening to make a separate peace, insisted on it in the interests of the balance of power, not even, one should note, for the sake of the Habsburg-Bonaparte dynastic bond. Prussia always demanded a severe reduction of French power and, after the Hundred Days, the detachment of Alsace-Lorraine in particular. Against a Napoleonic France, the tsar asserted the right of conquest, resisted a negotiated peace, and later intrigued to dictate the nature of the successor regime. Fearing that a Franco-Russian entente would result, Castlereagh joined Metternich in the (successful) effort to keep France in their camp. Thus France remained strong because, even before the fighting was over, the other powers were scrambling for the postwar affections of the late enemy, just as happened with Germany after World War II, only faster.

Even so, the treatment of France was mild primarily by the standards of 1871 or 1919 and on the assumption that 1792 was the proper point of departure. That has a nice legitimist ring, the year the Bourbon monarchy fell; but no other power wished its territory reduced to that measure, and it was granted to France because of competitive promises earlier made to leave France stronger than under its kings. Castlereagh for one considered it an unheard-of humiliation for a great power to be excluded, as France was, from a voice in the settlement beyond its frontiers. The eighteenth century had never been so predatory as that! Before taking his indignation too seriously, however, we should consider that he still wanted the French voice to be available in the balance against Russia.

As we all know, the French card came into play most conspicuously in the heat of the crisis over Saxony and Poland, allowing Talleyrand, the main booster of legitimacy, his finest hour. Schroeder's treatment of this subject is not only abstract but illogical and often factually wrong. We are told that, although Talleyrand's preaching was "freighted with" self-interest, his doctrine of legitimacy was sincere and widely shared.[9] Maybe, but it was not the grand universal principle usually claimed for it; rather, it was an exceedingly narrow and arbitrary doctrine, asserting merely that all thrones were legitimate that had not been signed away in treaty.[10] This formulation covered the reigning king of Saxony and also the ousted Bourbon king of Naples, the victim of conquest, but it made

[9] Schroeder, "Did the Vienna Settlement Rest," 696.

[10] Enno E. Kraehe, *Metternich's German Policy*, Volume 2: *The Congress of Vienna 1814–1815* (Princeton, N.J., 1983), 140–41.

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no allowance for duress: too bad that the hundreds of mediatized (that is, suppressed) houses and cities in Germany had acquiesced in the Imperial Recess of 1803, that the pope had signed various treaties dictating territorial losses, and almost everybody had had to accept the unforgiving results of lost battles.

But let us stick to the Saxon-Polish situation. If Russia and Prussia recognized no legitimist claims and if Metternich's and Castlereagh's first formula called for Prussian annexation of the kingdom, who was left to carry Talleyrand's banner? Answer: a few smaller German states like Bavaria and Saxe-Coburg, terrified at the threatened expansion of Prussia.[11] As for the major figures, how can we take seriously any later professions in defense of law and justice? Schroeder, resourceful as ever, has a riposte: namely, that with the collapse of Metternich's and Hardenberg's balance of power strategy in November 1814, "the real crisis" arose later because of Russia's continuing threat to the "political equilibrium."[12] If this expression denotes Russia's loyalty to her engagements with Prussia until the last minute, when the tsar backed down, what have we but balance of power maneuvering again?

But if, on the other hand, the crisis arose because Metternich feared a Polish constitution even more than Russia's territorial gains, why did he, contrary to Schroeder's version, first agree to accept such a constitution if Russia allowed the German powers stronger frontiers;[13] and why did the alliance partners of January 1815 not make their *sine qua non* the abandonment of a constitution rather than the survival of Saxony? Schroeder implies that it was simply a matter of facing facts, of obtaining what little could be had. Where is the gentlemanly concern for other states' security and status? His account of how the tsar forced his ally (in violation of all solemn understandings, one might add) to pay the price is absolutely correct, but this is a description of naked power politics at work, reminiscent of Britain's treatment of Frederick II in 1763,[14] not a quest for "political equilibrium," an expression never defined but apparently meant to be equated with the observance of a certain civility and a high-minded concern for other powers' rights and security, none of which induced Alexander to abandon his Polish constitution project or his territorial demands. Schroeder is right about one thing: the resulting system was highly unstable and threatened the central powers down to World War I, that is, long after the Polish constitution had vanished; but how this "confirmed and strengthened . . . political equilibrium," however defined, remains a mystery.[15]

The author's strained interpretation of this recurrent phrase creates so many problems and proves so difficult to apply with consistency, at least in reference to the Vienna settlement, that one wonders why he invests so much capital in it. The reason is crucial: his need to explain why, in the absence of a balance of power, hegemonic politics did not degenerate into naked power struggles and recurrent wars, not only at the Congress of Vienna but for most of the ensuing century. This

[11] Kraehe, *Metternich's German Policy*, 2: 266.
[12] Schroeder, "Did the Vienna Settlement Rest," 703.
[13] Kraehe, *Metternich's German Policy*, 2: 228–30.
[14] That is, in the treaty of Paris wherein Britain required France only to evacuate Prussian provinces but not to retrocede them, forcing Frederick to bargain for their return.
[15] Schroeder, "Did the Vienna Settlement Rest," 703.

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need arises, however, from a slippery use of the term "hegemony." The problem is not one of definition—Schroeder intends nothing more than a simple dictionary definition in which hegemony means domination—but its application to the Vienna settlement. Whether a system be "hegemonic" or "balanced," we need to be clear about the geographical limits in which it operates. It is one thing to say that the superpowers achieved hegemony in their separate spheres, the one on the high seas, the other in Eastern Europe and northern Asia, quite another to speak, as Schroeder does, of an Anglo-Russian hegemony over the whole. He himself explains that neither of these powers could conquer the other, which is to say that at a global level we are not talking about hegemony at all but the rivalry of the two powers. Perhaps, he concedes, but the rivalry was low key, the two powers had few points of friction, and in any case, on the few occasions that found them acting in unison, they easily had their way.

This picture is distorted. The rivalry, even if less than life-threatening to either of the powers, was real, keenly felt, and on Russia's side at any rate vigorously pressed. The tensions in Central Asia, Persia, and the Ottoman empire are too familiar to review here save to note that they began earlier than Schroeder suggests (not in 1828 but with the Anglo-Persian alliance of December 1814) and that Russia's vacillating preference for preserving the Ottoman empire was less an example of benevolent restraint than a practical calculation that indirect Russian preponderance in the whole was better than the annexation of a part—precisely the reasoning that Catherine II had applied to Poland in the 1760s. However that may be, it is part of Schroeder's argument that whatever rivalries existed outside of Europe, they could safely be pursued because of their condominium over Europe, where they respected the so-called political equilibrium. Let us return to that.

IT IS THE CONTENTION HERE that the Saxon-Polish crisis marked not the transition from power politics to political equilibrium but the point at which, stalemated (checked? balanced?) in the military realm, the superpowers had no choice but to pursue their goals by other than military means; and, as long as their contest continued, the middle powers could enjoy some degree of independence. On Britain's part, the task on the Continent remained, as always, to prevent the concentration of the Continent's resources under a single organizing authority—that is, to maintain a balance of power. By the same token, Tsar Alexander tried to do just that—not by conquest, which the lesson of the Triple Alliance put out of reach, but by cooperative arrangements embodied in such instruments as the Holy Alliance, the Troppau Protocol, and the several international congresses that he alone initiated, all of which in the end had the effect of drawing the Continental states closer together and further from Britain. He could probably have controlled Central Europe more effectively than he did by playing the two powers against each other and heeding the lesser states' appeals for his protection. For a time, he tried this in a gingerly way; but, by 1819, when he saw that this course was alienating the German powers and Britain as well, thus jeopardizing

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his larger pan-European projects, Alexander decided to permit Austria its German sub-hegemony after all, that is, by acquiescing in the Carlsbad Decrees. Shortly thereafter, he adopted a similar solution for Italy, allowing Metternich his unilateral incursion into the peninsula in exchange for the Troppau Protocol, which paid lip service to the principle of a collective, great-power right of intervention against revolution. Schroeder is right to call these results sub-hegemonies for they were imposed with the consent of both the true hegemons, but for opposite reasons: Britain for the sake of a Central European barrier to France and Russia, and Russia for the sake of Alexander's larger Continental goals designed to leap that barrier.

In the pursuit of these goals, meanwhile, the tsar endeavored to forge tighter links with the secondary sea powers. Portugal, always subject to British intimidation, proved to be out of reach. With the Netherlands, his success was modest, producing the marriage of his sister Anna to the young prince of Orange but never budging the Dutch from their scrupulous neutrality. With France, his leverage was greater. As noted, Russian courting of France began with the fall of Napoleon, encouraged a moderate peace, in large measure inspired the Holy Alliance, spurred the early evacuation of France, and produced numerous schemes for a formal entente, preferably sealed by a marriage with the duke de Berri.

Alexander's patronage of Spain was even more conspicuous, because it meant the steadfast support of the reactionary Ferdinand VII at a time when elsewhere he stridently encouraged liberals and nationalists. Recent scholarship as well as newly published Russian documents illuminate this relationship better than ever before.[16] On some issues, which involved complications with other clients, actual and prospective, the tsar sometimes disappointed Ferdinand, but in regard to Latin America he was a friend indeed. Apart from the conquest of India, perhaps, one could strike no greater blow to Britain's global position than to snatch the rebellious colonies away from exclusive British influence, especially British trade. This subject was on Alexander's agenda at every one of the post-1815 congresses. Material aid he rendered with the sale of Russian ships for transporting troops to the New World.[17] With the rebellion in Cadiz, however, the main question was now the fate of Spain itself. Although the tsar's plans for pan-European intervention ran aground at the Congress of Verona, the French invasion, which he had encouraged from the beginning, strengthened his ties with both countries as never before, routing British influence in Madrid and raising for Britain the specter of a new concerted attempt to recover the colonies. As we all know, Canning's swashbuckling response was to call "the New World into existence to redress the balance of the old,"[18] in the process grudgingly accepting the new

[16] Anna Maria Schop Soler, *Un siglo de relationes diplomaticas y commerciales entre España y Rusia 1733–1833* (Madrid, 1984); and U.S.S.R., Ministry of Foreign Affairs, *Vneshniaia politika Rossii xix i nachala xx veka: Dokumenty rossiiskogo Ministerstva inostrannykh del*, vols. 8–12 (Moscow, 1960–80).

[17] Contrary to legend, the ships were generally seaworthy and were sold rather than given to minimize the alarm in other world capitals. Schop Soler, *Un siglo de relationes diplomaticas*, 210–33.

[18] Address to Parliament, December 12, 1826, text in Harold Temperley, *The Foreign Policy of Canning 1822–1827: England, the Neo-Holy Alliance, and the New World* (London, 1925), 579–84.

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sub-hegemony proclaimed by the United States. Is that political equilibrium or balance of power at work?

On this note, we return to the question of nomenclature. While alluding to the protean character of "balance of power," Schroeder in practice adopts the severely predatory model of the eighteenth century as his standard of judgment. In the real world, however, including the eighteenth century, balance of power decisions rest on worst-case estimates. Nothing in the system requires that all states at all times scheme for advantage, expansion, and conquest or cease to balance the costs of war against domestic strain and hardship. Divided counsels, personal interests and capabilities, moral preferences, human prudence or incompetence, even an enlightened regard for other states' interests—these always operate and coexist within any system. Balance of power is at bottom a matter of perceived capabilities more than demonstrated intentions, and one is as hard to gauge as the other.

Since all peace settlements consist of some mixture of the above ingredients, we should also ask how the Vienna settlement compares to others. It was not based on the dynastic principle or on religion (the monarchical principle and the Holy Alliance notwithstanding) as had been usual until the Peace of Westphalia. In fact, except in the vaguest sense, it was not founded on ideology of any kind, not even the concept of restoration so monotonously associated with its name. It did not restore the mediatized houses or ecclesiastical estates in Germany or the Knights of St. John of Malta or all of the pope's former rights. Instead, it allowed most of Napoleon's creations to stand, and in France itself the Charter of 1814 was in part meant to protect Napoleonic institutions from counter-revolution. Neither was the settlement a one-sided *Diktat* like the peace treaties of 1866 and 1871, although these were tempered by Bismarck's fear of European intervention. Most emphatically, it was not based on the idea of national self-determination like the Versailles settlement of 1919, which according to Woodrow Wilson was expressly to repudiate "the great game, now forever discredited," of the balance of power.[19] What did distinguish the Vienna settlement was its relatively dispassionate appreciation of power, which discouraged utopian schemes and risky experimentation. Compare the attempt in 1919 to curb Germany by short-lived artificial restrictions with the Vienna formula of creating physical counterweights against France and then continuing intact the four-power coalition that had won the war—pure balance of power thinking even if not completely shared by a vindictive Prussia and an all-forgiving Russia.

Thus, on comparative grounds alone, it is reasonable to say that the Congress of Vienna was uncommonly attentive to the balance of power no matter how that term is defined. This is not to challenge Schroeder's compelling argument that each of the powers pursued hegemony in a sphere it regarded as its own, only to deny that there emerged a global or European hegemony, which he invents by making Britain and Russia out to be a single hegemon. Rather, as argued here,

[19] Address to Congress, February 11, 1918, text in Woodrow Wilson, *The Published Papers of Woodrow Wilson*, Ray Stannard Baker and William E. Dodd, eds., 6 vols. (New York, 1925–27), 5: 182–83.

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their relationship was adversarial, constituting a bipolar balance of power,[20] which in turn fostered the development of other regional spheres of influence that neither could easily menace without the risk of provoking a hostile coalition. If that polarity was more sedate, less nerve-racking than that after 1945, it was because the superpowers by force of circumstances possessed mutual security from each other, not the power of mutual annihilation.

[20] I am reminded of a recent contention that the resurgence of marriage in the United States is not based on the old father-knows-best relationship but on an uneasy "balance of power between husband and wife." If Paul Schroeder wishes to reply that the partnership nevertheless exists as an entity, I think I can afford to yield the point. See Abigail Trafford, "Domestic Tranquility: Modern Marriage in America and the Balance of Power," *Washington Post*, December 27, 1991.

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AHR Forum

A Political Science Perspective on the Balance of Power and the Concert

ROBERT JERVIS

POLITICAL SCIENTISTS sometimes treat historical scholarship the way Sergeant Friday treated witnesses: "just the facts, ma'am." This attitude is never less appropriate than when confronting the writings of Paul Schroeder.[1] Here we find arguments about important issues and theories of general political processes. Some of Schroeder's analysis is consistent with ideas developed by students of international politics; other parts represent major challenges. At a time when political scientists have shown renewed interest in the Concert of Europe both for its intrinsic significance and for its possible relevance in the post–Cold War era, it is particularly valuable to explore the differences between the Concert and the balance of power as seen by scholars in both disciplines.[2]

Some scholars may object that Schroeder's analysis is too sweeping, that it sees balance of power as one pattern when it should be seen as much more varied, that it draws excessively stark contrasts between balance and a managed system. Arguments on these questions can be fruitful but not definitive. Along with and usually inseparable from a sense of how ordered history is, scholars differ in their aesthetic senses of the attractiveness of parsimony and richness, just as some historians are lumpers and others are splitters. Some scholars strive for the kind of understanding that involves the fullest possible picture. In this light, few events closely resemble each other, and no single cause dominates any particular case. Others try to construct accounts that are much more parsimonious, in the sense of seeing a relatively large number of events—including ones that superficially appear quite different—as sharing crucial characteristics, of believing that one or two causes are primary in each case, and of suspecting that the same factors are at work in numerous instances. The price of doing so (too high for many

[1] My assignment is to discuss "Did the Vienna Settlement Rest on a Balance of Power?" *AHR*, 97 (June 1992): 683–706, but this comment applies to Schroeder's other writings as well.

[2] See, for example, Robert Jervis, "Security Regimes," *International Organization*, 36 (1982): 360–68; Jervis, "From Balance to Concert," *World Politics*, 38 (1985): 58–79; Charles Kupchan and Clifford Kupchan, "Concerts, Collective Security, and the Future of Europe," *International Security*, 16 (1991): 114–61; Gregory Gause, "Post-War Gulf Security: Hegemony, Balance, or Concert?" unpublished manuscript. It might be noted that interest in the Concert has been increased by articles on it by historians that have appeared in *World Politics*, the leading journal in the field: Richard Elrod, "The Concert of Europe: A Fresh Look at an International System," *World Politics*, 28 (1976): 159–74; Paul Schroeder, "The 19th-Century International System: Changes in the Structure," *World Politics*, 39 (1986): 1–26.

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historians) is to blur differences among cases and to leave out many parts of the story.

To develop parsimonious explanations, political scientists construct abstract models and theories. The most obvious question that we try to answer in this area is how international politics can maintain restraint and stability despite its being anarchic, that is, lacking a central authority above the states that has a monopoly on the legitimate use of force and can make and enforce binding agreements. Not only is the anarchical international system a permissive cause of wars in the sense that there is no authority to prevent them but, even in the absence of disputes over such values as territory, prestige, and religion, conflict is generated by the security dilemma. In other words, frictions are created because under most conditions the ways in which states seek to guarantee their own security (armies and alliances, for example) have the unintended consequence of making others less secure.[3]

From this perspective, what is most striking about international politics is what has *not* occurred—no single state has conquered all the others, wars rarely become total and usually end in negotiated settlements, losers rarely are divided up and usually become reintegrated into the system. Even when wars are frequent, there are important forms of restraint and three types of stability. First, many patterns recur. One of the pleasures of teaching introductory international politics is assigning sections of Thucydides and having students coming in saying, "Why, it is just like the Cold War." Second, and part of the cause of these similarities, the anarchical nature of international politics has not been transformed. Thus, while there is a (disputed) field of study of "political development" dealing with domestic politics, there is no similar subfield of international politics. Third, there is stability in the technical sense of negative feedback—the movement of critical variables in one direction generates forces that limit these changes and returns the system to something like its previous equilibrium.

The theory of the balance of power comes into play here.[4] The growth of a nation's power, if it becomes great enough to menace other strong states, will be at least partially self-defeating; the attempt to dominate the international system will call up a counter-balancing coalition that will restrain the state. According to balance of power theory, restraint and stability are not goals of national leaders but are by-products of the pursuit of narrow self-interest. More precisely, as long as states seek to survive, can ally with any other state in response to external incentives, and are willing to resort to war if need be, then none will be able to dominate and the other forms of restraint and stability will occur. As I will discuss below, if these conditions do not hold, the pattern will be different.

[3] John Herz, "Idealist Internationalism and the Security Dilemma," *World Politics*, 2 (1950): 157–80; Herbert Butterfield, *History and Human Relations* (London, 1951); Robert Jervis, *Perception and Misperception in International Politics* (Princeton, N.J., 1976), 62–90. Thucydides was perhaps the first to note this phenomenon. This does not mean that greed, aggression, and misunderstandings do not cause war but only that wars can occur in their absence through the desire for security.

[4] As Schroeder notes, there are actually several balance of power theories. The best treatment is Inis Claude, *Power and International Relations* (New York, 1962). Also see Ernst Haas, "The Balance of Power: Prescription, Concept, or Propaganda," *World Politics*, 5 (1953): 442–77; and the special issue of *Review of International Studies*, 15, no. 2 (1989). The version of the theory I am talking about is drawn from Claude's "automatic" version, best elaborated in Kenneth Waltz, *Theory of International Politics* (Reading, Mass., 1979). Also see Stephen Walt, *The Origins of Alliances* (Ithaca, N.Y., 1987).

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In this view, many of the obvious sources of moderation need not be present—there is little need for international law, a sense of common community, or shared concepts of rights and justice.[5] One attraction of the theory—which may strike some as perverse—is that this proposition contradicts common sense. Also contrary to common sense is the argument that restraint and stability can result even though the actors' behavior is not moderate; indeed, despite claims to the contrary, none of the states need seek balance.[6] The theory is also attractive because it explains, with a few simple assumptions, a great deal of behavior that has taken place over tens of centuries under widely differing circumstances. The operation of the balance thus shows the ability of the anarchical international system to override variations in statesmen's personalities, transnational technologies and climates of opinion, and the domestic characteristics of states.

While balance of power theory cuts against many of our intuitions, it is built on fairly straightforward logic. Stability and restraint arise through the process of conflict in a way similar to the operation of Adam Smith's invisible hand. The desire for survival and autonomy leads states to join together temporarily in order to stop a potential hegemon even if they have severe conflicts with each other and even if many would like to be hegemons themselves. The other restraints and puzzles mentioned earlier—the fact that wars are usually not total and that losers are not divided up—also follow from the dictates of self-interest within the constraints imposed by the anarchical system. Since any state can ally with any other, states do not have permanent friends and enemies. To fight a total war or divide up the loser is then dangerous on two counts. First, the state may need the current adversary as an ally in the future. Second, while the state would gain territory and wealth from dividing up the loser, others might gain more than it would, thus putting the state at a disadvantage in subsequent conflicts. The same logic explains why small or weak states rarely get divided or swallowed. No one wants anyone else in the system to get too strong; a state that conquers one of its small neighbors is likely to be in a better position to menace others; the invitation to join with others in dividing a weak state will be scrutinized with an eye to relative rather than absolute gains.

The result, then, is a relatively moderate outcome despite—or, rather, because of—the fear and greed of the individual states. Restraint is internalized only to the extent that states may anticipate that to behave too forcefully will be to lead others to mobilize against them. (This exception leaves room for much cooperative behavior and so makes it difficult to disconfirm the theory.[7]) The general picture is of a world in which states constantly take advantage of any opportunity to make competitive gains at others' expense, consult with others only when they believe that the results will be in their immediate interest, and freely employ tactics of surprise and duplicity. There is little sense of a community of nations, of shared responsibility, or of the desire and need to preserve the system. I think most political scientists would join me in agreeing with Schroeder that this picture does

[5] These are stressed in Hedley Bull, *The Anarchical Society* (New York, 1977).

[6] David Hume, "Of the Balance of Power," in Charles Hendel, ed., *David Hume's Political Essays* (Indianapolis, Ind., 1953), 142–44; Edward Gulick, *Europe's Classical Balance of Power* (New York, 1967); Hans Morgenthau, *Politics among Nations*, 6th edn. (New York, 1985), 214–15.

[7] See Schroeder, "Did the Vienna Settlement Rest," 699.

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not describe the Congress of Vienna and the international politics prevailing for some period thereafter. Edward Gulick reconciles behavior at Vienna with the balance of power by misrepresenting the latter. Behavior was moderate, but that is what the balance calls for: "restraint, abnegation, and the denial of immediate self-interest." What is required is "the subordination of state interest to balance of power."[8] While one can define "balance of power" in this way, it does not fit with common usage and is not helpful.

Other scholars need not be disturbed by the change in behavior at Vienna because their theories are at a higher level of generality. Thus a follower of Kenneth Waltz could argue that the crucial element was that, after 1815, as before it, no single state dominated the system and that France was not dismembered.[9] The new pattern of diplomacy is at a level of detail that international politics theory does not and need not address. I find this position unsatisfactory. Parsimony is purchased at too high a price: too much is put aside and too little can be explained.

But two converging lines of political science research, while probably excessively stark for most historians, do speak to the shift from balance to Concert. First, while the assumptions supporting balance of power theory are normally unproblematic, two of them are likely to be undermined by a war against a potential hegemon.[10] After the Napoleonic wars, war was not seen as a normal tool of statecraft, and France, though reintegrated into international politics, was seen not as a country like any other but as one that was especially likely to disturb the status quo. Furthermore, the experience of fighting a long, difficult war forged unusual bonds among the states. As a result, whether or not the basic idea of checking power by power was abandoned, the normal practices of diplomacy were. Despite significant conflicts of interest among them, all the states shared the important interest of avoiding large wars and the need to maintain the new, more cooperative arrangements.[11] Further, states tended to be restrained in anticipation of the consequences of overambitious behavior; a heightened awareness of the security dilemma reduced it. International politics was then characterized by an unusual degree of moderation, respect for others' interests, and attempts to work with as many other states as possible.

This does not mean that the theory of the balance of power is incorrect. Rather, it no longer applied because its assumptions were not met. Ironically, the transformation was brought about by the sort of cataclysmic war that represents the balance of power in its starkest form—the formation of a grand coalition to block a state that would otherwise dominate. Thus, without being overly dramatic, one can argue that the balance of power has within it the seeds of its own destruction.

[8] Gulick, *Europe's Classical Balance of Power*, 33, 304.

[9] Indeed, for Waltz, the fate of France or any other great power would not matter as long as there were enough states remaining to continue the multipolar system.

[10] The following paragraphs are drawn from Jervis, "From Balance to Concert."

[11] Schroeder argues that this was less true for the two relatively invulnerable states, Britain and Russia; Schroeder, "Did the Vienna Settlement Rest," 686–90. But even they were less willing to fight than they had been in the eighteenth century.

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The second and related argument shares with balance of power the starting points of anarchy and the security dilemma. But it notes that while the extremely conflictual pattern of the balance is likely to emerge when many or all of the states seek to expand, an alternative is possible when states "merely" want to protect themselves. International politics sometimes resembles what is called a Prisoner's Dilemma (PD). In this scenario, two men have been caught red-handed committing a minor crime. The district attorney knows that they are also guilty of a much more serious offense. He tells each of them separately that if he confesses and squeals on his buddy, he will go free and the former colleague will go to jail for thirty years. If both of them refuse to give any information, they will be prosecuted for the minor crime and be jailed for thirty days; if they both squeal, plea-bargaining will get them ten years. In other words, as long as each criminal cares only about himself, he will confess to the more serious crime no matter what he thinks his colleague will do. If he confesses and his buddy does not, he will get the best possible outcome (freedom); if he confesses and his buddy also does so, the outcome will not be good (ten years in jail), but it will be better than keeping silent and going to jail for thirty years. Since both can see this, both will confess.[12] Paradoxically, if they had both been irrational and kept quiet, they would have gone to jail for only a month.

This is not a bad model for much of international politics and, indeed, for much of social life.[13] The actor's first choice is to prevail over or exploit the other. (For convenience, this is labeled DC, meaning the actor defects from a friendly coalition with the other, while the other cooperates with him.) Thus states often want more arms than their neighbors and wish to use tougher bargaining tactics in order to gain an upper hand in disputes. The second choice is mutual cooperation (CC)—for example, compromise on a lower and mutually agreed-upon level of arms. The third choice is mutual defection (DD)—unrestrained competition, an arms race, or perhaps a war. The worst outcome would be for the state to cooperate with the other while the other exploits it (CD)—cases in which the state abides by an arms control agreement while the adversary does not, the state follows a conciliatory path in a dispute while the adversary maintains a hard line.

But DD—the actor's third choice—is required by narrow self-interest and rationality only if the game is played just once; if the game is to be repeated for years, this is not the case. While defecting on any particular play of the game will

[12] It is interesting to note that in the real world this does not seem to happen as often as the theory implies it should: Brian Forst and Judith Lucianovic, "The Prisoner's Dilemma: Theory and Reality," *Journal of Criminal Justice*, 5 (1977): 55–64. The reasons for the discrepancy probably are several, including the danger of punishment for those who squeal, which modifies the assumption of anarchy.

[13] The following paragraphs are based on Robert Jervis, "Cooperation under the Security Dilemma," *World Politics*, 30 (1978): 167–214; Robert Axelrod, *The Evolution of Cooperation* (New York, 1984); Robert Keohane, *After Hegemony* (Princeton, N.J., 1984), the special issue of *World Politics*, 38, no. 1 (1985), which is also available as Kenneth Oye, ed., *Cooperation under Anarchy* (Princeton, 1985). For critical discussions of this literature, see Robert Jervis, "Realism, Game Theory, and Cooperation," *World Politics*, 40 (1988): 317–49; Joseph Grieco, "Anarchy and the Limits of Cooperation," *International Organization*, 42 (1988): 485–508; Steve Weber, *Cooperation and Discord in U.S.–Soviet Arms Control* (Princeton, 1991). For an alternative, but not completely unrelated, discussion of cooperation that is less parsimonious (or less stereotyped), see Alexander George, Philip Farley, and Alexander Dallin, eds., *US-Soviet Security Cooperation* (New York, 1988).

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both protect the actor if the rival/partner defects and brings significant gains if the rival does not, it will also decrease the chances of cooperation in the future. Thus, starting from assumptions similar to those of the balance of power, this model indicates that a different, more cooperative, way of dealing with anarchy and the security dilemma can emerge under certain circumstances. Cooperation is most likely when the gains from exploiting the other (DC) and the costs of being exploited (CD) are both relatively low, when mutual competition (DD) is much worse than mutual cooperation (CC), when exploitation is not much better than mutual cooperation, and when being exploited is not much worse than mutual competition.

Because it is in the interest of each state to cooperate only if others are doing so, prospects for cooperation are increased if states can tell if others are living up to their agreements (what is known as "transparency") and if they can act effectively in the event that others are not. In a parallel way, the security dilemma will be less of a barrier to cooperation if defensive postures are more potent than offensive ones and if observers can distinguish between the two. Under these circumstances, states can provide for their own security without greatly menacing others, and mutual security will be possible. To reap the advantages of cooperation that these circumstances permit, actors are often well served by a contingent strategy that matches what they do with what others are doing. The most obvious of these strategies is reciprocity—actors will cooperate if others cooperate and defect if others defect. In effect, this strategy removes both temptation and fear by making it impossible for either side to exploit the other over a long period. The choice is then between conflict and mutual cooperation, and, in the PD model, cooperation is preferred.

Even in the fuller treatments, this sketch, because it is parsimonious, is highly simplified. But I think it captures the flavor of the Concert pattern. The costs of war (DD) were seen as higher than they had been in the eighteenth century (in part because even a successful war might lead to revolution[14]) and the value of mutual cooperation similarly increased as the new system met the basic needs of the states and held out the chance for not only avoiding large-scale violence but also for solving important problems. Although the hope for unilateral competitive advantage (DC) was not absent, to make such short-run gains appeared foolish because it would not be too long before others responded with hostility. The higher levels of communication and more frequent meetings among national leaders increased transparency, lowered the level of debilitating suspicions that plague many attempts at cooperation, and made it less likely that any statesman could think that he could successfully cheat on understandings with others. Furthermore, statesmen had grounds for believing that others shared these preferences, thus facilitating long-run reciprocity. As a result, in addition to making direct and immediate bargains and trades, states could refrain from taking advantage of others' temporary weaknesses in the expectation that, when the tables were turned, others would behave similarly toward them rather than allow the system to return to the more conflictual balance of power pattern.

[14] Schroeder, "Did the Vienna Settlement Rest," 700.

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Although the two lines of analysis I have presented are couched in terms different from those used by Schroeder, I see them as consistent with much of what he says. But there is a divergence at two important points. First, Schroeder here hints and elsewhere has argued extensively that, contrary to the view outlined earlier, the balance of power did not yield stability and restraint.[15] While some logic might indicate that the invisible hand of competition would limit excessive national ambitions and maintain the system on some sort of even keel despite—or because of—high conflict and wars, in practice, this picture is no more accurate than is the idealized view of a laissez-faire domestic economy. Some states—such as Britain and Russia—were sufficiently strong and geographically well positioned as to be almost invulnerable, able to dominate many areas of the world and meddle in others. For less advantaged states, particularly Austria, security simply could not have been gained by balance of power rules in which territory, influence, and prestige could only be gained at the expense of others. In the absence of a sense of common interests, respect for small powers and intermediary bodies, and related mechanisms and modes of organization, there would be so much violence that the system would simply tear itself to pieces. In part, the two views can be reconciled by arguing that even though Schroeder's balance of power world is extremely harsh and violent, it is stable in the minimal sense that no one state will dominate and that some of the other restraints noted earlier will hold. But the picture Schroeder draws does not easily fit with the expectations generated by the models that most American students of international politics find useful and appealing.

Second, the transformation of the system from balance of power to equilibrium that Schroeder portrays involves more profound changes than those I have sketched. In my outline, the actors are still driven by narrow self-interest, conceived of in ways quite similar to those prevailing under the balance. The numerous and significant changes in behavior are produced by changes in the dangers and opportunities presented by the environment. For Schroeder, however, the change penetrated to states' goals, outlooks, values, and the system's organizing principles. States came away from the decade of war with reduced ambitions and a willingness to seek adjustment to, rather than domination of, rivals. As Schroeder remarks in discussing the conflicts of the mid-1840s, "Austria was no longer working to keep France out of Italy and France was no longer trying to throw Austria out."[16] The fact that statesmen did not take every opportunity to expand represented not only changed calculations but, more important, a change in the conception of self-interest. Thus, after 1815, Russia no longer used alliances with Austria and Prussia "to keep both German powers insecure, dependent on Russia, and locked in rivalry with each other," as had been the case before the Napoleonic wars.[17]

But it is too simple to argue that the post-1815 system was moderate because the

[15] Paul Schroeder, "The 19th Century System: Balance of Power or Political Equilibrium?" *Review of International Studies*, 15 (1989): 135–53; Paul W. Schroeder, *The Transformation of European Politics 1787–1948* (New York, forthcoming).

[16] *Ibid.*, chap. 17, p. 26.

[17] Schroeder, "Did the Vienna Settlement Rest," 688; also see 701–02.

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actors were satisfied with the status quo.[18] The hopes for expansion were mediated by a change in values and outlook. Statesmen thought more in terms of the international system and what was necessary to keep it functioning.[19] Central was "a mutual consensus on norms and rules, respect for law, and an overall balance among the various actors in terms of rights, security, status, claims, duties, and satisfactions rather than power."[20] Instead of seeking "all possible compensations and indemnities simply in order to keep up with or get ahead of its neighbor and rival . . . , [states focused on] the needs of Europe [which] were understood more in legal, moral, and socio-political terms than in terms of simple power."[21] This analysis resembles "regime theory," in which political scientists have stressed how shared principles, norms, and procedures can permit sustained cooperation despite significant conflicts of interest.[22] But Schroeder places greater stress on law, legitimacy, and rights: "the Vienna equilibrium represented ideally a balance between what each state needed or claimed to need in order to fulfill its proper role and function within the European family and what that family as a whole considered necessary and proper."[23]

The change in values and beliefs about how politics can and should be conducted was very great. It cannot easily be accounted for by parsimonious models that treat the ideas and preferences of actors as given and proceed from there to work out the expected interaction patterns.[24] The conception of self-interest expanded, and statesmen came to believe that menacing states could best be contained by keeping close ties to them. A necessary, though probably not sufficient, cause for the transformation of world politics then was a transformation in thinking about world politics. The new ways of thinking grew out of the failure to gain security through more conventional policies in the Napoleonic wars, but the learning process was not a simple reflection of that experience. Insight, creativity, and independent thinking played a greater role than they do in most political science treatments of learning.[25] Ideas had real autonomy; they were not the direct and predictable consequence of changed material conditions.

The change cannot, however, be seen as operating only at the level of individual states and statesmen. The new approach had to be adopted by most if not all of them if it was to succeed. Any state that abandoned balance of power on its own would be rebuffed if not exploited by others that did not. While the balance of power is individualistic in its premise that each state will try to advance its own interests, the Concert requires that states think more broadly and rely more on others, who are expected to be doing so as well. When states are restrained more

[18] Paul Schroeder, "Containment Nineteenth-Century Style: How Russia Was Restrained," *South Atlantic Quarterly*, 82 (1983): 6–9.

[19] For further discussion, see Paul Schroeder, "The Transformation of Political Thinking, 1787–1848," in Jack Snyder and Robert Jervis, eds., *Coping with Complexity in the International System* (Boulder, Colo., forthcoming).

[20] Schroeder, "Did the Vienna Settlement Rest," 694.

[21] Schroeder, "Did the Vienna Settlement Rest," 697.

[22] For a good summary, see Stephen Krasner, *International Regimes* (Ithaca, N.Y., 1983).

[23] Schroeder, "Did the Vienna Settlement Rest," 698.

[24] For further discussion, see Jervis, "Realism, Game Theory, and Cooperation," 324–28.

[25] The best collection is George Breslauer and Philip Tetlock, *Learning in U.S. and Soviet Foreign Policy* (Boulder, Colo., 1991).

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by the desire to maintain ties to friends and allies than by threats from enemies,[26] a high level of mutually reinforcing behavior is required. Most of the states need to contribute to the building and maintaining of a pattern of relations that sustains them in their moderation.

This kind of system, unlike the balance of power, calls for explicit and self-conscious management. Each state not only depends on others doing their share but must also regulate its own behavior so that it is not excessively disturbing. Leaders must attend not only to whether others are threatening them but also to whether their actions—or inactions—threaten the vital interests of others and thereby menace the entire system. Thus the great powers in general and Britain in particular helped bring on World War I when they failed to respond to events such as Romania's anti-Austrian alignment by asking themselves "how . . . this new, crucial development [could] be managed . . . , harmonized with the overall European balance, incorporated into the prevailing international system, without raising the already fearful strains upon that system to the point of explosion."[27] But we still know too little about the conditions that are most propitious for this kind of system maintenance. Supportive domestic interests and structures probably play a large role, especially in the economic arena,[28] but political science has not fully engaged either this question or the closely related one of the dynamics of a system based on dealing with potential enemies by grouping with rather than balancing against them.

Indeed, a skeptic may wonder whether the analysis is complete because the system as described is vulnerable to a return to a more predatory stance on the part of one or more of the major states. Schroeder sees this decay as leading to World War I. It is dangerous for a state to be concerned with rights and responsibilities if others focus on power and seek to exploit weakness or even ignore the needs of the system. Even though maintaining such a system is less demanding than establishing it,[29] the task still is not easy; although there are significant tools states can use against those who would exploit the system—such as exclusion from it[30]—these may not be powerful enough to cope with those who seek its overthrow. In the balance of power, the stability of the system is maintained by states following narrow, short-run self-interest. Wars may be frequent, but at least the imperatives that leaders have to follow are relatively clear and simple. In a Concert system, especially of the type described by Schroeder, states and national leaders must have wider concerns, a longer-run perspective, and greater wisdom.

[26] See Schroeder, "Containment Nineteenth-Century Style"; and Paul Schroeder, "Alliances, 1815–1945: Weapons of Power and Tools of Management," in Klaus Knorr, ed., *Historical Dimensions of National Security* (Lawrence, Kan., 1976), 227–62.

[27] Paul Schroeder, "Romania and the Great Powers before 1914," *Revue roumaine d'histoire*, 14 (1975): 52–53.

[28] Krasner, *International Regimes*; Robert Keohane and Joseph Nye, Jr., *Power and Interdependence* (Boston, 1977); Helen Milner, *Resisting Protectionism* (Princeton, N.J., 1988).

[29] Keohane, *After Hegemony*.

[30] Schroeder, "Did the Vienna System Rest," 700.

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AHR Forum

Was There a Reformed Balance of Power System or Cooperative Great Power Hegemony?

WOLF D. GRUNER

PAUL SCHROEDER'S ESSAY ON THE VIENNA SETTLEMENT AND THE BALANCE OF POWER touches on two major fields of research: the actual distribution of power and operation of the system, and the language and practices of the Vienna settlement. He argues that we need not redefine the concept of balance of power with respect to the Vienna system of 1815 or understand it differently. For him, that settlement did not represent a particular, modified kind of balance of power. He believes rather "that any balance of power interpretation of the Vienna settlement is misleading and wrong. Its essential power relations were hegemonic, not balanced, and a hegemonic distribution of power, along with other factors, made the system work."[1]

As far as the language and the practices of the Vienna system are concerned, Schroeder discusses the seemingly plausible reasons for calling the system of Vienna a balance of power system, referring to the language being used by statesmen and writers of the period, the success of the international system established at Vienna, and the assumption that a hegemonic system could not suddenly come about and provide peace and stability. These perceptions he tests against his own evidence. He concludes that the case against the balance of power interpretation of the Vienna system is clear, stating that "eighteenth-century balance of power rules and practices produced predatory, destabilizing hegemony. The Vienna era's equilibrist rules and practices promoted benign, stabilizing kinds of hegemony."[2]

I would argue instead for a characterization of the international system established in 1815 as a reformed, multipolar, and intertwined balance of power system with built-in checks. The Vienna system was not designed and set up as a hegemonic great power system, despite the fact that the great powers played a

[1] Paul W. Schroeder, "Did the Vienna Settlement Rest on a Balance of Power?" *AHR*, 97 (June 1992): 684; compare Paul W. Schroeder, "Germany and the Balance of Power: Past and Present," Part 1, in Wolf D. Gruner, ed., *Gleichgewicht in Geschichte und Gegenwart* (Hamburg, 1989), 134–39. My own ideas have been dealt with in more detail in Wolf D. Gruner, "Deutschland und das europäische Gleichgewicht seit dem 18. Jahrhundert," in *Gleichgewicht in Geschichte und Gegenwart*, 60–133; and in my essays on Germany as the heartland of Europe, discussing problems of the European balance of power, new forms of international cooperation, and the role of small and middle states in the international system, Wolf D. Gruner, *Deutschland mitten in Europa* (Hamburg, 1992).

[2] Schroeder, "Did the Vienna Settlement Rest," 702.

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leading role in the process of establishing a new European system. It was, rather, a carefully balanced multipolar system composed of states differing in size, power, financial and economic resources, and influence. It constituted a stable system of great powers and intermediary bodies bound together by a common European culture and traditions and accepting set rules for their bilateral and multilateral relations to safeguard their political and territorial independence within a new system of public law. Thus the driving forces and influences that led to a functioning international system are far more complex than Schroeder wants us to believe.

In the space allotted me, I shall focus on one example. The destruction of the Holy Roman Empire of the German nation as a political and legal body necessitated a reconstruction of Central Europe (*Mitteleuropa*) in order to avoid a power vacuum. Any new order for Central Europe had to take into account the political, territorial, economic, and social changes that had occurred in Europe since 1789. The German middle states, which had become sovereign in 1806, were bound to play an important role.

If we still want to apply the balance of power concept in historical research and not place the "abused" and "misleading" concept "on the shelf," we have to keep in mind the parameters for this type of system: "A balance of power apparatus could not function without the existence of a state system . . . [I]t could not operate without a minimal homogeneity of political attitude among the participating state units and a common concern to protect the system; it could not maintain itself without a limitation of warfare to preserve the constituent state components."[3] This characterization applies equally to superpowers, great powers, medium-sized powers, and small states and provides the best method for analyzing intergroup relations, shifts, and changes within the international system.[4]

Since the second half of the eighteenth century, international politics has been a system of change influenced by a set of major determinants. This set (described below) can be used as a tool for describing and analyzing not only the multipolar balance of power system established in 1815 but also the changes taking place in the course of the nineteenth century—which were to some extent the result of the independent existence of the German and European intermediaries—concerning structure, mutual obligations, adaptability, and accepted leading principles and guidelines. The positive notion of the balance of power concept enabling systemic change, mutual security, and peace and stability in the nineteenth century was replaced by a negative one. Balance of power stood for a static and failed system restricting member states from pursuing their legitimate national claims.[5] For the period of "real systemic change" from a mechanical eighteenth-century type balance of power toward the multipolar system of 1815, as well as for the period of the replacement of the balance of power concept by new forms of international

[3] Richard N. Rosecrance, *Action and Reaction in World Politics: International Systems in Perspective* (Boston, 1963), 25 and following.

[4] See Ernst B. Haas, "The Balance of Power: Prescription, Concept or Propaganda? The Re-emergence of the Balance of Power Concept," in *World Politics*, 5 (1952): 442–77.

[5] See, for example, W. Maurenbrecher, "Die deutsche Frage 1813–1815," in *Preussische Jahrbücher*, 27 (1871): 39–60.

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cooperation, the following determinants should be considered: the national setting of a political unit (economic, intellectual, cultural, and social framework); its overall power (political, financial, material, and labor resources); objective and subjective national security interests; national interests, aims, and claims; structure and development of the international system; geography; national self-conception and identity; and, last but not least, the perception of national and international influences and determinants by the political elite. The interaction of these determinants is vital for any balance of power and international system.[6] My use of the proposed determinants will make clear that Schroeder and I differ in our understanding of the period, especially as regards Central Europe, and about methodological and terminological issues.

The Vienna system of 1815 formally marked the end of the epoch that followed the French Revolution of 1789. The period had been characterized by the breakdown of the old European state system that had developed since the sixteenth century and the breakdown of European public law and decomposition of the Holy Roman Empire, which, ever since the tenth century, had played a role within the European system. Also important were fundamental changes taking place since 1789 in the political, social, economic, and territorial structure of Europe. These changes affected agriculture, trade, industry, and the means of communication and were partly furthered, partly impeded, by the long period of continuous war. Indeed, the wars had a far-reaching impact on almost all spheres of life. These changes could hardly be ignored in any peace settlement.[7]

The states of southern Germany had in 1806 become "sovereign" de jure when the Holy Roman Empire de facto ceased to exist. The monarchs in these states endeavored to retain the "national" sovereignty established in 1806 in any new "German Empire," just as they had done as allies of Napoleon in the Confederation of the Rhine.[8] Even before 1806, *raison d'état* required these monarchs to initiate reforms in order to save their political units from extinction. The reforms launched in Baden, Bavaria, and Württemberg between 1804 and 1814 may not have been completely successful, but such efforts enabled the states of South Germany to become self-confident and territorially enlarged polities capable of

[6] I have discussed these determinants in more detail and in a European and German context in Gruner, *Gleichgewicht in Geschichte und Gegenwart*, 65 and following.

[7] See Karl Otmar Freiherr von Aretin, *Vom Deutschen Reich zum Deutschen Bund* (Göttingen, 1980), 114 and following; Helmut Berding, ed., *Privatkapital, Staatsfinanzen und Reformpolitik im Deutschland der napoleonischen Zeit* (Ostfildern, 1981); Helmut Berding, Etienne François, and Hans-Peter Ullmann, eds., *Deutschland und Frankreich im Zeitalter der Französischen Revolution* (Frankfurt am Main, 1989); Elisabeth Fehrenbach, *Von Ancien Régime zum Wiener Kongress* (Munich, 1981); Wolf D. Gruner, *Die deutsche Frage: Ein Problem der europäischen Geschichte seit 1800* (Munich, 1985), chaps. 2, 3; Gruner, "Deutschland und das europäische Gleichgewicht seit dem 18. Jahrhundert," 60–133, esp. 97 and following; Wolf D. Gruner, "Deutschland zwischen Revolution, Reform und Restauration 1770–1830," in *Tijdschrift voor geschiedenis*, 102 (1989): 368–400; Hans A. Schmitt, "Germany without Prussia: A Closer Look at the Confederation of the Rhine," in *German Studies Review*, 6 (1983): 9–39; Mack Walker, *German Home Towns: Community, State, and General Estate, 1648–1871* (Ithaca, N.Y., 1971), 185 and following; James J. Sheehan, *German History 1770–1866* (Oxford, 1989), 235 and following.

[8] See Eberhard Weis, ed., *Reformen im rheinbündischen Deutschland* (Munich, 1984); Wolf D. Gruner, "Die Deutschen Einzelstaaten und der Deutsche Bund," in Andreas Kraus, ed., *Das Land in der deutschen Geschichte: Bayern—Ausnahme oder Beispiel? Festschrift für Max Spindler zum 90. Geburtstag* (Munich, 1984), 19–36.

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playing an important role in any new European and German order. In view of these changes, the postwar system had to be more than just a product of classical eighteenth-century cabinet diplomacy and a restoration of the *status quo ante* if it was to meet the challenge posed by the existing socio-political systems and ruling elites.[9]

In a "reconstruction of Europe," Great Britain—the rising world power and dominant industrial nation—was to be a major player.[10] Britain's strategy for peace cannot be explained simply in terms of security, trade interests, and foreign affairs motives, nor should its long and enduring war against revolutionary and Napoleonic France be labeled a quest for hegemony only.[11] Apart from the dynamics of the international system itself, serious attention ought to be paid to domestic and world economic influences and to the development of industrial societies, factors neglected in Schroeder's analysis.[12] The British government was for the most part convinced that the European situation determined the country's capacity to function not only in the domestic sphere but also in the international arena. In order to avoid the dangers of overtaxing Britain's strength, the government sought a European and international order that would eliminate potential conflicts as much as possible. Thus British interests demanded a reformed balance of power based on a multipolar international system, created by, among other things, peaceful shifts in the "natural interests" of its members in Europe. To establish such a system, the European middle and small powers were to stabilize the European order and the regional subsystems.

For Britain, the realization of these aims was a necessary prerequisite of economic prosperity, financial efficiency, domestic stability, and peaceful opportunities for an evolutionary development within its existing socio-political system. Thus Britain in its own interest was working for a peace based on a "double balance of power" (*Doppeltes Gleichgewicht*) and was hoping to make this an enduring balance by means of a "double peace" (*Doppelte Friedenssicherung*). In view of the major impact of war on the social, political, and economic system of Britain and the changes the country underwent during the wars, Schroeder is mistaken in stating that the British war aim of restoring a balance of power in Europe "means nothing—or rather, shows that their respective definitions of the European balance of power were hegemonic, as they had always been."[13]

[9] See Eberhard Weis, *Der Durchbruch des Bürgertums 1776–1847* (Berlin, 1978), 11 and following, a perspective that British studies neglect despite its being true for Britain also. Compare Derek McKay and H. M. Scott, *The Rise of the Great Powers, 1648–1815* (London, 1983).

[10] See Wolf D. Gruner, "The British Political, Social and Economic System and the Decision for Peace and War: Reflections on Anglo-German Relations 1800–1939," in *British Journal of International Studies*, 6 (1980): 189–218, esp. 197 and following; and Wolf D. Gruner, *Grossbritannien, der Deutsche Bund und die Struktur des europäischen Friedens im frühen 19. Jahrhundert*, 2 vols. (Munich, 1979), 1: 56 and following.

[11] For a more complex and structural approach, see Gruner, *Grossbritannien, der Deutsche Bund und die Struktur des europäischen Friedens*; and Wolf D. Gruner, "'Peace and Prosperity': Anmerkungen zur gesellschaftlichen und wirtschaftlichen Dimension britischer Politik am Ende der napoleonischen Kriege," in Ferdinand Seibt, ed., *Gesellschaftsgeschichte: Festschrift für Karl Bosl zum 80. Geburtstag*, 2 vols. (Munich, 1988), 2: 204–22.

[12] Compare Gruner, *Grossbritannien, der Deutsche Bund und die Struktur des europäischen Friedens*, 1: 56 and following; Paul M. Kennedy, *The Rise and Fall of the Great Powers* (London, 1988), 126 and following; F. H. Hinsley, *The Fall and Rise of the Modern International System* (Canberra, 1981).

[13] Schroeder, "Did the Vienna Settlement Rest," 690. For a detailed analysis of Britain's policy, see

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The territorial changes that took place during the Napoleonic period demanded a reconstruction of Europe and a readjustment of power. In this context, the rearrangement of Central Europe was decisive for the creation of a stable European and international balance of power after the Napoleonic wars. A reformed balance of power system was a major goal of those seeking a new and durable means to peace, stability, security, and prosperity. They saw this as mandatory since, after the experience with the hegemonical, universal monarchy of a dominating state—Napoleonic France—there was no alternative to an international system based on a reformed balance of power.[14] In building such an order, consideration had to be given to the best means for guaranteeing the political, economic, and military independence of the European states. The same was true of the security interests of the actors within the international system, France included.

Writers and statesmen of the period demanded the establishment of a "free political system" that would respect the independence and existence of its members and would be prepared to defend this independence. The demand for a new public law in Europe and a reformed balance was more than mere rhetoric of officials and statesmen at conferences and in their chancelleries or of pamphleteers and paid political propagandists.[15] People were convinced that after the war governments could establish a stable and perfect system of states based on permanent organizational structures, common responsibility and cooperation, binding rules, arbitration mechanisms, and common actions and sanctions against violators of international law. These ideas received strong support from a large majority of German states.

The foundation of a new "German Empire"—the new national bond of the Germans—was to play a key role. A reformed European balance of power could only function if there was a "German balance of power," that is, if the new political constitution and the boundaries of the "empire" guaranteed a balanced system of the German great powers (Austria, Prussia), the powers of the second order (*Mittelstaaten*) such as Bavaria, Hanover, Baden, Württemberg, and Saxony, and the minor German states like the Free City of Frankfort and the Hanse towns. It was essential that the new empire be strong enough to resist all attacks from east or west. At the same time, however, it should not become so powerful as to be able to launch attacks on its neighbors.[16]

Gruner, *Grossbritannien, der Deutsche Bund und die Struktur des europäischen Friedens*, 1: 76 and following.

[14] See Arnold Ludwig Herrmann Heeren, *A Manual of the History of the Political System of Europe and Its Colonies . . .*, 2 vols. (Oxford, 1834), based on the 5th German edn. of 1809, 2: 401 and following.

[15] See, for example, Friedrich von Gentz, *On the State of Europe before and after the French Revolution*, John Charles Herries, trans. (London, 1802); von Gentz, *Fragmente aus der neuesten Geschichte des politischen Gleichgewichtes in Europa* (St. Petersburg, 1806); [Anonymous], *Betrachtungen über die Wiederherstellung des politischen Gleichgewichtes in Europa* (Leipzig, 1814); and the files on the period 1806–1815 in the archives of the respective German states (Haus- Hof- und Staatsarchiv Vienna, Staatskanzlei; Bayerisches Hauptstaatsarchiv Munich MA; Niedersächsisches Hauptstaatsarchiv Hannover).

[16] Wolf D. Gruner, "Europäischer Friede als nationales Interesse: Die Rolle des Deutschen Bundes in der britischen Deutschlandpolitik 1814–1832," in *Bohemia*, 18 (1977): 96–128, esp. 100 and following.

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There were three solutions under discussion for the future national bond of the Germans: first, the reestablishment of a reformed, more centralized Old Empire under Austrian leadership. Constitutionally, this model seemed to be the easiest way to restructure Central Europe. But this concept met with fierce opposition from Prussia and all the German middle states except Hanover. The non-German great powers regarded this solution as a replacement of French hegemony over Europe by an Austrian one. The second proposal was for an Austro-Prussian condominium over Germany. This plan was opposed by the German middle states and other European powers that had participated in the deliberations at Paris and Vienna. Moreover, the Paris peace treaty of 1814 ruled that there should be a "federal bond" among the German states in a new German system. The third option called for a structure more like a "confederate" (*staatenbündisch*) or "federal" (*bundesstaatlich*) type. A confederate solution seemed to be the best possible choice at the time from the standpoint of a functioning international system of European interests, a balanced political system, and the varying levels of modernization in Germany.

At the conferences at Chaumont, Paris, and Vienna, the middle states had seen fit to subscribe to only a rather loose federal bond. From their point of view, the articles of the Treaty of Paris providing for a "federal bond" (*lien fédératif*) for Germany had to be interpreted according to the plans for Italy. An instruction sent to the Bavarian envoy in London makes this clear: "The independence of the Italian states, based on an alliance and mutual guarantee of the sovereigns . . . is the only suitable constitutional bond for Germany."[17] Like Bavaria, Württemberg also favored the "Italian model," a league of states that would not curtail Bavaria's sovereign rights.[18] Whereas Baden and Württemberg believed that their treaties of accession to the anti-Napoleonic alliance would permit them to wait and see what the constitutional developments in Germany were and decide whether the results were compatible with their sovereign national interests and European ambitions, Bavaria decided to cooperate and to pursue its interest through constitutional proposals and determined bargaining.[19] Thus, in the final stages of the constitutional discussion, Bavaria acquired a key role. If Bavaria, the largest and most powerful middle state in southern Germany, had not joined a German Confederation, the entire German and European system would not have worked, for in that case the other middle states in the south would have kept aloof from the confederation and formed a South German League.[20] An independent *Sonderbund* (Special League) was not in the interest of a stable and balanced reconstruction of Central Europe. There was an additional danger if the south did not join the confederation: the position of the German great powers would be strengthened at the expense of the other member states. This would have prevented a German balance of power.

[17] Bayerisches Hauptstaatsarchiv Munich, Gesandtschaft London 279, Max Joseph (Montgelas)-Pfeffel, 6/15/1814 (enlarged instruction).

[18] Württembergisches Hauptstaatsarchiv Stuttgart, E 70 Bü. 12.

[19] Gruner, *Grossbritannien, der Deutsche Bund und die Struktur des europäischen Friedens*, 1: 343 and following, 415 and following.

[20] *Ibid.*, 415 and following. See also Peter Burg, *Die deutsche Trias in Idee und Wirklichkeit* (Stuttgart, 1989), 70 and following.

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The German Confederation established on June 8, 1815, was able to fulfill its German and European functions for more than fifty years in part because the confederation, as a national bond of the German nation, linked together all the states descended from the Old Empire, forming a special type of intermediary body. Through its constitution and institutions, it provided protection and security to its members against incursions by German and European powers. The federal constitutional framework enabled the confederation to overcome or at least to neutralize the antagonism of both German and European great powers that had endangered the European system in the eighteenth century. On this point, I agree with Schroeder's notion that the confederation solved the "great foreign policy problem of Germany . . . , enabling German great powers, middle states, small states, free cities, estates, princes, Catholics, and Protestants to live harmoniously inside" the new Germany for many decades.[21]

The existence of the German Confederation in the center of Europe also guaranteed the stability necessary for the entire European system. The confederation was to become the "keystone" of the European order and the still largely Eurocentric international system created by the Treaties of Paris and the Congress of Vienna. The constitutional framework of the confederation assigned to German small and middle states a constitutive role for a stable and functioning balance of Germany and Europe as well as for the regional balance affected by the Vienna system, for example for southern and northern Germany. Finally, its constitution and political structure allowed the German Confederation to play a central role in Europe's system of security against potential aggression or ambitions of either France, the new superpowers, or the German great powers.

The German Confederation did not degenerate into an additional power base for Austria or Prussia. It did not become a sub-hegemony in the kind of cooperative hegemonical system that Schroeder believes existed. For persuasive reasons, Prussia and Bavaria failed in their respective territorial claims, which would have assigned to them a quasi-hegemonical role in southern or northern Germany. The military institutions of the German Confederation provided the means for a collective European security and for the incorporation of vulnerable states such as Denmark and the Netherlands into its system of defense.

IN SUMMING UP, I would argue that the Congress of Vienna laid the groundwork for a postwar order capable of promoting the peaceful development of Europe. It seemed to provide the right answer: an international system based on a multipolar balance of power in which great, middle, and small powers assumed a constitutive role. The system was flexible. Systemic change became a permanent feature of the order of Vienna. The great powers assumed the role of a steering committee and developed institutional means for handling crises without becoming a cooperative hegemonic force. One of the great advantages of the system of Vienna was its replacement of a mechanical balance of power system based on the

[21] Paul W. Schroeder, "The European International System, 1789–1848: Is There a Problem? An Answer?" Paper presented at the Woodrow Wilson International Center for Scholars, Colloquium, March 19, 1984, p. 10.

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five great powers with a complex, multipolar system of built-in checks and balances. Despite many interesting and convincing points made by Paul Schroeder, I cannot accept his description of the international order established in Vienna in 1815 as a system based on a cooperative great power hegemony. Nevertheless, his challenging contribution to the history of international relations will advance debate and possibly provide new insights into the latter half of the nineteenth century.

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AHR Forum

A Mild Rejoinder

PAUL W. SCHROEDER

SINCE THE *AHR* has already allotted generous space to both the commentators and me, I feel I should not attempt a detailed rebuttal, even where inclined to, but merely indicate briefly what the lines of reply might be, leaving the details to my forthcoming book.

I would like to thank all the commentators for their kind words and for the substantial measure of agreement even from the critics.

I have no further reply to Robert Jervis at all—only additional thanks for another elegant demonstration of why we international historians should pay more attention to political scientists and theorists of international relations like him.

I fear that where Enno Kraehe and I disagree, we simply read the evidence very differently. My reaction to his spirited and witty critique (which he may find infuriatingly patronizing, but it is not meant that way) is that he does as well as anyone can in sustaining a balance of power interpretation, but that it finally will not work. Kraehe's first main thesis, that the transactions and negotiations at Vienna were basically a kind of balance-of-power realpolitik for which the talk about legitimacy, rights, and law served as a fig leaf, seems to me unable to account either for the process or the results. Of course the ideas were narrowly and self-interestedly conceived, arbitrarily and inconsistently applied. That is the nature of politics, of life, of law itself. That the Vienna statesmen were determined to achieve a balance of rights, worked to adjudicate claims rather than impose solutions by force majeure, and break with the old politics by establishing a new comprehensive basis of guaranteed treaty rights for everyone, so that international politics would now be bounded by law, seems to me beyond serious dispute.

His second main argument is that there was a world balance of power and a real rivalry for world power between Britain and Russia. Once again, I simply do not see it. To make another of the sweeping generalizations he found irritating: there was a serious Anglo-Russian rivalry all through the revolutionary and Napoleonic era, covert at some periods, overt in others (1800–1801, 1807–1812), and Russia lost that contest. It lost a war with Britain decisively in 1807–1812 before it fought and won the war with France. Another serious Anglo-Russian world struggle would begin with the Crimean War, partly as a result of it. Between 1815 and 1854, there was no such struggle worth talking about. What one sees from

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1815–1820 is partly the dying echo of the real competition of 1792–1815, taken far more seriously by Russian diplomats and men on the spot than by Alexander, and still less by Castlereagh or Liverpool; partly competition for prestige, again taken more seriously by some Russians than by the British; and mainly Alexander's genuine zeal to lead everyone into the brave new world of Christian peace. Why historians persist in seeing cunning Russian political purposes behind this, when nothing is more clear than Alexander's willingness to sacrifice or subordinate concrete Russian goals and gains in order to maintain the grand alliance and be recognized as its leader—when Capodistrias criticized precisely this willingness and Metternich and Castlereagh consciously exploited it—I do not understand. As for Spain and the Latin American colonies, it was always Spain trying to use Russia and other countries for its purposes, not the other way round. The only opponents the British took even half-seriously were the United States and France. France and Russia, knowing the British position was unassailable, were trying mainly to control Britain within a restraining partnership over Latin America, not compete for supremacy. Moreover, after 1824, the pattern of Russia's quest for a partnership with Britain over the heads of the Continental powers, especially on the Near East, is surely unmistakable. Canning refused to see a Russian danger even where it existed to a certain degree in Persia, and Palmerston talked about it but actually did far more cooperating with Russia than combating it.

As for Wolf Gruner, I apologize for neglecting his major contributions to the international history of this period, along with much else, in this essay, though not, I hope, in the book. I do not see that Gruner and I actually disagree except on terminology, where to be sure I still insist on mine. Almost everything he says about the structure and workings of the 1815 system, especially the crucial importance of the German Confederation and the role of small states, I would not merely concede but insist on. That the system had to balance other things besides power and that socioeconomic and domestic political factors were very important is equally true. That Britain played a leading role and that its policy, based on changed interests, was drastically different from that of the eighteenth century is another point of agreement (though this applies better to Castlereagh, Wellington, and Aberdeen than Canning and Palmerston). What Gruner calls "a carefully balanced multipolar system" seems to me just what I call "political equilibrium." Since "political equilibrium" is the literal translation of the central nineteenth-century term, and conveys what we are both talking about as "balance of power" does not, I think we should use it.

There is, however, a real difference stated in his title: Gruner's "reformed balance of power system" versus my supposed "cooperative great power hegemony." Here I may have misled both Gruner and Kraehe, not, I think, by direct statement but through emphasis. In arguing against a balance of power view and insisting that the distribution of power was hegemonic, I may have given the impression that I think the system worked, operated, achieved the results it did *by means of* cooperative great power hegemony. That is clearly not true. Britain and Russia did not normally settle issues in Europe by imposing their agreed-upon solutions on everyone else. Austria and Prussia did not even regularly do so in Germany, or Austria in Italy. What I do contend is that the 1815 system, so far as

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distribution of power was concerned, was hegemonic, and that whenever in the crunch power factors became decisive, the hegemonic pattern became evident. Normally, however, the various hegemonies were benign, tolerable, and inactive. What made the system work and gave it a remarkable durability and stability was political equilibrium—working for a balance of satisfactions, rights, assurances, obligations, security, status, within certain bounds set by law. The distinction seems to me both tenable and useful. For example: throughout the Cold War, the general world international system rested on a balance of power and also worked by means of it. The Soviet sphere both rested on clear-cut hegemony and operated by it. The American sphere in Western Europe and North America, however, rested clearly on American hegemony in terms of power, and whenever power factors became decisive (Cuba, Suez, Nicaragua, Panama, the Persian Gulf), American hegemony emerged fairly nakedly. Yet the American system worked at least most of the time in much of the area by political equilibrium—one big reason why it is still around and the Soviet system is not. The case of the 1815 system is of course not as clear-cut as these, but the distinction, I contend, still applies.

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Gender and the Politics of Class Formation: Rethinking German Labor History

KATHLEEN CANNING

CLASS FORMATION IS ONE OF THE MOST IMPORTANT MARKERS of the economic and social transformation of nineteenth-century Europe, of the dissolution of feudal estate society and the rise of a modern, industrial urban society. "Class" is also a keyword in the vocabulary of social and labor history, one that occupies a central place in a process of rethinking and recasting the conceptual framework of social history that is currently under way in the United States, Britain, and France.[1] The emergence of gender as a category of historical analysis and the growing importance of language in the theory and practice of social history have undermined the stability of the historical and analytical vocabulary of social and labor history, especially of the concepts of experience, agency, and class. The "linguistic turn" combined with the theoretical innovations of feminist history have resulted in a "disorienting" epistemological crisis in social history, during which the concepts of class and class formation have been challenged and redefined.[2]

Earlier versions of this essay were presented to the Program on the Comparative Study of Social Transformation at the University of Michigan, at the 1989 meeting of the Social Science History Association, and to the conference, "The *Kaiserreich* in the 1990s: New Research, New Directions, New Agendas," held at the University of Pennsylvania in 1990. I would like to thank Jane Caplan, David Crew, Laura Downs, Geoff Eley, Karin Hausen, Carol Karlsen, Vernon Lidtke, Hubert Rast, Sonya Rose, Adelheid von Saldern, David Scobey, Bill Sewell, Wilfried Spohn, Margaret Somers, and finally, the anonymous *AHR* reviewers for critical questions, helpful suggestions, and fruitful discussions of this essay.

[1] Patrick Joyce, "In Pursuit of Class: Recent Studies in the History of Work and Class," *History Workshop*, 25 (Spring 1988): 175; and Lenard Berlanstein, "Working with Language: The Linguistic Turn in French Labor History; A Review Article," *Comparative Studies of Society and History*, 33 (April 1991): 439. See also Raymond Williams, *Keywords: A Vocabulary of Culture and Society* (New York, 1983).

[2] See, for example, Joan W. Scott, "Gender: A Useful Category of Historical Analysis," *AHR*, 91 (December 1986): 1053–75; John Toews, "Intellectual History after the Linguistic Turn: The Autonomy of Meaning and the Irreducibility of Experience," *AHR*, 92 (October 1987): 879–907. On the linguistic turn in German history, see Peter Schöttler, "Historians and Discourse Analysis," *History Workshop*, 27 (Spring 1989): 37–65; and the following essays in *Central European History*, 22 (September–December 1989): Michael Geyer and Konrad Jarausch, "The Future of the German Past: Transatlantic Reflections for the 1990s," 229–59; Jane Caplan, "Postmodernism, Poststructuralism, and Deconstruction: Notes for Historians," 260–78; and Isabel V. Hull, "Feminist and Gender History through the Literary Looking Glass: German Historiography in Postmodern Times," 279–300. On the epistemological crisis, see Bryan Palmer, *Descent into Discourse: The Reification of Language and the Writing of Social History* (Philadelphia, 1990), 78–86, in which Palmer describes being "quite shaken" by Scott's deconstructive challenge. Berlanstein's view of the "linguistic turn" is more sympathetic but also points to the disorienting effects of Scott's work on the field of social history in his "Working with Language," 439.

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"Class" in its Marxist or Weberian versions has usually designated a relationship to the process of production and a corresponding social identity. Since the publication of E. P. Thompson's classic work, *The Making of the English Working Class*, in 1963, many historians have also embraced the view of class as a cultural relation. During the 1980s, the works of historians William H. Sewell, Jr., Gareth Stedman Jones, William Reddy, and Patrick Joyce brought the cultural dimensions of class into sharper focus. Sewell and Stedman Jones, for example, shifted the emphasis of labor history away from socioeconomic causality toward a new emphasis on political language, ideology, rhetoric, and representation, away from a notion of class as a "social fact" to one of class as a postulated "social identity."[3] William Reddy's *Money and Liberty in Europe* emphatically rejected the "simple pairings of social classes with political factions" and dispensed with a view of the "means of production as an unambiguous marker of class identity."[4] In his recent *Visions of the People*, Patrick Joyce distinguishes carefully between the "consciousness of class" and the consciousness of *a* class, emphasizing, for example, that the working class in Lancashire was more likely to embrace populism than to manifest "class consciousness." Although Joyce concludes that class was "only one of the ways in which people patterned and gave meaning to the social order," he explicitly "pull[s] back from the verge of denying class."[5] Instead, the culturalist approach to labor history seeks to redefine class by historicizing it, by attending to the "actual terms in which contemporaries talked about the social order."[6] Thus, while class is retained as an analytical category, the culturalist labor histories of the 1980s recast it as a remarkably elastic concept.

At the same time, recent attempts to establish the relationships among and between the distinct social identities of gender, race, ethnicity, and class have created a different kind of "crisis of the concept of class,"[7] one that reveals the ways in which class subsumes these identities and differences. Women and gender have presented a perplexing problem for histories of class since the late 1960s, when feminist scholarship began to challenge prevalent historical and sociological

[3] Geoff Eley, "Is All the World a Text? From Social History to the History of Society Two Decades Later," in Terence J. McDonald, ed., *The Historic Turn in the Human Sciences* (Ann Arbor, forthcoming 1992), 18–19. On the shift away from socioeconomic causality, see Berlanstein, "Working with Language," 428–29. The works referred to here are William H. Sewell, Jr., *Work and Revolution in France: The Language of Labor from the Old Regime to 1848* (Cambridge, 1980); Gareth Stedman Jones, *Languages of Class: Studies in English Working Class History, 1832–1982* (Cambridge, 1983). A valuable discussion of class in German labor history can be found in David Crew, "Class and Community: Local Research on Working-Class History in Four Countries," in Klaus Tenfelde, ed., *Arbeiter und Arbeiterbewegung im Vergleich: Berichte zur internationalen historischen Forschung* (Munich, 1986), 279–80. For a discussion of experience, see: William H. Sewell, Jr., "How Classes Are Made: Critical Reflections on E. P. Thompson's Theory of Working-Class Formation," in Harvey J. Kaye and Keith McClelland, eds., *E. P. Thompson: Critical Perspectives* (Philadelphia, 1990), 50–77; Joan Wallach Scott, "The Evidence of Experience," *Critical Inquiry*, 17 (Summer 1991): 773–97.

[4] William Reddy, *Money and Liberty in Europe: A Critique of Historical Understanding* (1987; Cambridge, 1990), 30–31.

[5] Patrick Joyce, *Visions of the People: Industrial England and the Question of Class, 1848–1914* (Cambridge, 1991), 5, 13–23, 329–31. See also Patrick Joyce, *The Historical Meanings of Work* (Cambridge, 1987).

[6] Joyce, *Visions of the People*, 1; see also Reddy's exploration of the many possible ways in which the concept might be redefined, all of which, in his view, "are equally successful, none of which is evidently superior to any of the others"; Reddy, *Money and Liberty*, 30.

[7] Reddy, *Money and Liberty*, 30.

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paradigms. Labor historians and political theorists often attempted to fit women into prevalent notions of class, some by arguing that women formed a separate class of their own, others by examining the ways in which (female) gender presented an obstacle to the formation of a cohesive class unit. Still others circumvented the dilemma of "sex and class" by ascribing to women the same class position, identity, and interests as those of their fathers or husbands. Feminist historians sought to transcend this theoretical impasse by formulating a "doubled vision" of society, one that emphasized the simultaneity of sex and class in the lives of both men and women.[8] In dissolving the myth of a "natural" division between public and private, between women and men, and in analyzing the ways in which these divisions were "socially constructed and socially imposed," this doubled vision prepared the way for the shift toward "the self-conscious study of gender," even if it left the concept of class intact.[9]

Parallel to the culturalist labor historians' critical engagement with class during the last decade, feminist labor historians explored the role of sexual difference in shaping the process of working-class formation. British historian Sally Alexander's study of women's work in nineteenth-century London led her to issue a radical call in 1984 for "feminist history . . . to emancipate itself from class as the organizing principle of history, the privileged signifier of social relations and their political representations."[10] Yet this call may have been somewhat premature, at least in the United States, for an "emancipation from class" necessitated the development of alternative conceptual tools. Joan Scott's influential article, "Gender: A Useful Category of Analysis," published in 1986, established a theoretical framework for the shift from women's history to gender history. In refining the analytical concept of gender, Scott made a convincing case for its usefulness to historians in general and also for its potential to destabilize and displace established categories like class.[11] Her subsequent critique of Gareth Stedman Jones's *Languages of Class* made explicit the ways in which "concepts such as class are created through differentiation" and argued that "gender becomes so implicated in concepts of class that there is no way to analyze one without the other."[12] The appearance in the next year of *Family Fortunes* by Catherine Hall

[8] The term "doubled vision" is from Joan Kelly's important essay, "The Doubled Vision of Feminist Theory," Joan Kelly, *Women, History, and Theory: The Essays of Joan Kelly* (Chicago, 1984), 51–64, first published in *Feminist Studies*, 5 (Spring 1979): 216–27. See also Judith L. Newton, Mary P. Ryan, and Judith R. Walkowitz, eds., *Sex and Class in Women's History* (London, 1983), esp. "Editors' Introduction," 1–15.

[9] Joan Kelly, "The Social Relations of the Sexes," in Kelly, *Women, History, and Theory*, 6, refers to the social construction of these divisions. Newton refers to the "self-conscious study of gender" in the "Editors' Introduction" to *Sex and Class*, 4.

[10] Sally Alexander, "Women, Class, and Sexual Differences in the 1830s and 1840s: Some Reflections on the Writing of a Feminist History," *History Workshop*, 17 (Spring 1984): 133–49. See also Sally Alexander, *Women's Work in Nineteenth-Century London* (London, 1983); Barbara Taylor, *Eve and the New Jerusalem: Socialism and Feminism in the Nineteenth Century* (New York, 1983); Patricia Hilden, *Working Women and Socialist Politics in France, 1880–1914: A Regional Study* (New York, 1986). Jean H. Quataert, *Reluctant Feminists in German Social Democracy, 1885–1917* (Princeton, N.J., 1979); and Laura Strumingher, *Women and the Making of the Working Class, Lyon, 1830–1870* (St. Albans, Vt., 1978) addressed some of the same issues during the late 1970s.

[11] Scott, "Gender: A Useful Category."

[12] Joan Scott, "On Language, Gender and Working-Class History," in Scott, *Gender and the Politics*

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and Leonore Davidoff offered a powerful historical example of the ways in which gender and class worked together in shaping middle-class identity in nineteenth-century England.[13]

This process of rethinking class, of exploring its cultural dimensions, of analyzing it in relation to gender, race, and ethnicity, which characterizes recent British, French, and American labor historiography, has had little or no impact thus far on German labor history. Far from an epistemological crisis or a new interrogation of established concepts and categories, Germany's preeminent labor historians have recently begun a massive project of synthesizing and summarizing the findings and accomplishments of this expansive field in a new and definitive series titled *History of Workers and the Workers' Movement in Germany since the End of the Eighteenth Century*.[14] Indeed, German labor history, as an elemental part of the "historical social-science" that was founded in Germany during the 1960s and 1970s, remains confined within a Weberian paradigm of modernization. Central to this paradigm is an *Entwicklungs- und Verlaufsmodell* of class formation, a model that outlines the progressive advancement of the various stages of class formation, which is shaped less by human actors than by structures and processes (for example, by changing market relations and the expansion of wage labor).[15] Thus the story of German working-class formation is most often analyzed as *Strukturgeschichte* (structural history) and has only recently begun to incorporate *Erfahrungsgeschichte* (the history of the experiences of the participants in this process). Aside from this recent concession to the history of experience, German labor history has remained remarkably impermeable not only to the "linguistic turn" but also to the achievements of women's history and gender history and to the important insights and challenges of the anthropologically oriented *Alltagsgeschichte*, a history of everyday life that dissolves the boundaries between workplace, household, and community in its exploration of experience and identity formation.[16]

of History (New York, 1988), 60. This essay was first published in *International Labor and Working Class History*, 31 (1987): 1–13.

[13] Leonore Davidoff and Catherine Hall, *Family Fortunes: Men and Women of the English Middle Class, 1780–1850* (Chicago, 1987). An interesting discussion of the book can be found in Judith Newton, "*Family Fortunes*: 'New History' and 'New Historicism,'" *Radical History Review*, 43 (1989): 6–11.

[14] Gerhard A. Ritter, ed., *Geschichte der Arbeiter und der Arbeiterbewegung in Deutschland seit dem Ende des 18. Jahrhunderts*. The volumes that have appeared thus far include vols. 1 and 2 by Jürgen Kocka, *Weder Stand noch Klasse: Unterschichten um 1800* (Bonn, 1990); *Arbeitsverhältnisse und Arbeiterexistenzen: Grundlagen der Klassenbildung im 19. Jahrhundert* (Bonn, 1990); and vols. 9, 10, and 11 by Heinrich August Winkler, *Von der Revolution zur Stabilisierung, Arbeiter und Arbeiterbewegung in der Weimarer Republik 1918 bis 1924* (Bonn, 1984); *Der Schein der Normalität, Arbeiter und Arbeiterbewegung in der Weimarer Republik 1924 bis 1930* (Bonn, 1988); *Der Weg in die Katastrophe, Arbeiter und Arbeiterbewegung in der Weimarer Republik 1930 bis 1933* (Bonn, 1987). Additional volumes by Ritter, Kocka, and Klaus Tenfelde are due to appear in the next several years.

[15] Kocka, *Weder Stand noch Klasse*, 33. See also Konrad Jarausch's assessment of German social history in his essay, "Towards a Social History of Experience: Postmodern Predicaments in Theory and Interdisciplinarity," *Central European History*, 22 (September–December 1989): 427–43.

[16] For a provocative example of *Alltagsgeschichte*, see Alf Lüdtke, "Organizational Order or *Eigensinn*? Workers' Privacy and Workers' Politics in Imperial Germany," in Sean Wilentz, ed., *Rites of Power: Symbolism, Ritual, and Politics since the Middle Ages* (Philadelphia, 1985), 303–33. Important discussions of this approach can be found in David Crew, "*Alltagsgeschichte*: A New Social History from Below?" *Central European History*, 22 (September–December 1989): 394–407; Geoff Eley, "Labor History, Social History, *Alltagsgeschichte*: Experience, Culture, and the Politics of the

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In the United States and England, feminist history has played a pivotal role in dissolving the "grand narrative histories" of state institutions, national events, socioeconomic structures, of class and class formation.[17] In Germany, by contrast, the relationship between feminist history and historical social science is marked by "mutual distancing."[18] As a result, many of the historical monographs on women's work, everyday lives, and political movements have been produced in relative isolation from the mainstream of historical social science. Many social and labor historians, in return, regard women's history as too specialized to be relevant to the discipline in general or to their synthetic histories of work, working-class formation, or *Gesellschaftsgeschichte* (history of society).[19] The recent shift to *Geschlechtergeschichte* (gender history), which professes more explicit theoretical aims than women's history and which obviously seeks to engage "mainstream" history, has changed little in this regard. Many German social historians continue to resist the notion that the "magic weapon of gender history," as one of their eminent representatives scornfully termed it, might be relevant to their own enterprise.[20] Binary oppositions like private-public or family-factory continue to pervade recent accounts of class formation, and the conceptual framework of German social history remains closed to the potentially disruptive effects of gender.

This "mutual distancing" between German labor history and the history of women and gender has had several important consequences. The most obvious is that the experiences of male industrial workers continue to shape the theory and practice of German labor history. Also, gender is either wholly absent or figures in only a minor way in theoretical models and historical accounts of working-class formation. Yet the shortcomings of German labor history cannot be remedied merely through an *ergänzende Geschichtsschreibung*, by retrieving the rest or the other (the women's) side of the story. In fact, feminist historians of Germany have recently pointed out that this kind of historical enterprise merely serves to reinforce their "outsider status."[21] Instead, they emphasize the important task of integrating gender history into *Gesellschaftsgeschichte*. Dorothee Wierling, for example, argues that the success of gender as a category of historical analysis

Everyday—A New Direction for German Social History?" *Journal of Modern History*, 61 (June 1989): 297–343; and Dorothee Wierling, "Alltagsgeschichte und Geschichte der Geschlechterbeziehungen," in Alf Lüdtke, ed., *Alltagsgeschichte: Zur Rekonstruktion historischer Erfahrungen und Lebensweisen* (Frankfurt, 1989), 169–90.

[17] Hull, "Feminist and Gender History," 279.

[18] Ute Frevert, "Klasse und Geschlecht—Ein deutscher Sonderweg?" in Logie Barrow, Dorothea Schmidt, and Jutta Schwarzkopf, eds., *Nichts als Unterdrückung? Geschlecht und Klasse in der englischen Sozialgeschichte* (Münster, 1991), 262.

[19] Frevert, "Klasse und Geschlecht," 261–62; Eve Rosenhaft, "Geschichten und ihre Geschichte: Ein Nachwort," in Barrow, *Nichts als Unterdrückung?* 248. Women and gender appear only in disparate footnotes in Hans-Ulrich Wehler's otherwise comprehensive *Deutsche Gesellschaftsgeschichte*, of which volumes 1 and 2 have appeared thus far (Munich, 1987). Kocka's *Weder Stand noch Klasse* and *Arbeitsverhältnisse* investigate the living and working conditions of female servants, agricultural workers, and employees in domestic and factory textile production only to conclude that the nature of their employment excluded them from the process of class formation.

[20] Hans-Ulrich Wehler, "Der deutsche Weg: Geoff Eleys Revisionsversuche," *Frankfurter Allgemeine Zeitung*, 71 (February 25, 1991). Wehler uses the polemical term "die Wunderwaffe der Geschlechtergeschichte."

[21] Frevert, "Klasse und Geschlecht," 262; Rosenhaft, "Geschichten und ihre Geschichte," 248.

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hinges on this very process of integration, by which social historians incorporate gender into their conceptual framework and gender history constitutes itself as an essential part of *Gesellschaftsgeschichte*.[22]

This study seeks to analyze prevalent paradigms of class formation, to critique their neglect of gender, and to explore the implications of bringing gender into the existing models. I will suggest that the two forms of absence—theoretical and historical-empirical—are related, and propose to evaluate the theoretical concepts of class and class formation in terms of a historical case study of male and female textile workers in Germany between 1880 and 1930. This attempt to insert gender as a category of historical analysis and women as historical subjects into prevalent models of class formation results not in the seamless integration Wierling imagines but in the transformation of the model itself.

DESPITE THE GROWING INTEREST OF HISTORIANS in the intersections of class with identities of gender, race, ethnicity, and religion, these concepts have had a negligible role in recent theoretical debates among historians and sociologists on working-class formation. Even recent bids to revise both old Marxist and newer Thompsonian orthodoxies, such as the 1986 collection of essays, *Working-Class Formation: Nineteenth-Century Patterns in Western Europe and the United States*, edited by Ira Katznelson and Aristide Zolberg, overlook or subsume these categories.[23] The absence of women and gender in prevalent models of class formation is rendered even more provocative by the prominence of both women and ideologies of gender in the recent literature on middle-class formation. As the studies by Mary Ryan on the United States, Bonnie Smith on France, Leonore Davidoff and Catherine Hall on England, and Ute Frevert on Germany have shown, home and hearth are central locations of middle-class formation.[24] The ideology of domesticity, which ascribed sharply differentiated roles to men and women in the public and private spheres, was a key aspect of the social identity of the German *Bürgertum* and the Victorian middle classes. The site of working-class formation, by contrast, was the predominantly male sphere of industrial production, and

[22] Wierling, "Alltagsgeschichte," 173.

[23] Ira Katznelson and Aristide R. Zolberg, eds., *Working-Class Formation: Nineteenth-Century Patterns in Western Europe and the United States* (Princeton, N.J., 1986). The essays in this volume seek to revise both Marxist orthodoxies and the "culturalist-experientialist" approach of E. P. Thompson. It has remained an influential and valuable collection since its publication, not least because of its theoretical and comparative approach to the historical problem of class formation in France, Germany, and the United States. Notable exceptions to the charge of neglecting gender, ethnicity, religion, and race in theoretical and historical investigations of working-class formation include Vernon L. Lidtke, "Burghers, Workers and Problems of Class Relationships 1870 to 1914: Germany in Comparative Perspective," in Jürgen Kocka, ed., *Arbeiter und Bürger im 19. Jahrhundert* (Oldenbourg, 1986), 29–46; Jean Quataert, "The Politics of Rural Industrialization: Class, Gender, and Collective Protest in the Saxon Oberlausitz of the Late Nineteenth Century," *Central European History*, 20–23 (June 1987): 91–124; and Wilfried Spohn, "Religion and Working-Class Formation in Imperial Germany, 1871–1914," *Politics and Society*, 19 (1991): 109–32.

[24] Mary P. Ryan, *Cradle of the Middle Class: The Family in Oneida County, New York, 1790–1865* (Cambridge, 1981); Bonnie G. Smith, *Ladies of the Leisure Class: The Bourgeoises of Northern France in the Nineteenth Century* (Princeton, N.J., 1981); Davidoff and Hall, *Family Fortunes*; Ute Frevert, *Bürgerinnen und Bürger: Geschlechterverhältnisse im 19. Jahrhundert* (Göttingen, 1988). See also Mary Poovey, *Uneven Developments: The Ideological Work of Gender in Mid-Victorian England* (Chicago, 1988).

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the universalizing claims of "class" effaced women's paid and unpaid labor. While sexual difference is viewed as a constitutive factor in middle-class formation, it is disguised and obscured in most historical accounts of working-class formation.

I intend to reevaluate prevalent theories of class formation from the perspective of gender, to analyze in theory and in one specific historical context the complex relationships between gender and class. My focus is the "levels approach" to class formation, as outlined in its theoretical dimensions by Ira Katznelson and in its historical dimensions by German historian Jürgen Kocka in the essay collection *Working-Class Formation*.[25] German labor historians first became familiar with the "levels approach" to class formation with the East German publication of Hartmut Zwahr's study of Leipzig metal workers in 1978.[26] Kocka formulated a similar four-tiered model of class formation in his *Lohnarbeit und Klassenbildung*, one that resonates in his essay in *Working-Class Formation* and that appears to coincide with Katznelson's conceptualization in most points.[27] In dividing the process of class formation into a series of levels and inserting levels of social and cultural formation between economic and political class formation, advocates of the "levels approach" challenge the old orthodox Marxist "class-in-itself-and-class-for-itself" teleology. At the same time, they offer something clearer than E. P. Thompson's notion of "experience" as a mediating factor between production or capitalist structure and the process of class formation.

Despite the common framework of levels or stages in the process of class formation, it is important to point to several significant differences between Zwahr on the one hand and Kocka and Katznelson on the other. According to Zwahr's model, the various levels represent a lineal, chronological development. Level one or economic formation, during which the capitalist economy developed and wage labor became the principal form of manual labor, brings about the second level of social formation, at which developing webs of family, marital, social, and community ties bound workers together within occupational groups and across traditional barriers of trade and skill. The progressive development of levels one and two created the conditions for level three: the emergence of a class-conscious proletariat imbued with the ideology of Marxism. While Katznelson and Kocka propose similar versions of levels one and two, their models offer a much more nuanced approach to "political" class formation, which in Zwahr's version is marked by the founding of a class-conscious labor union or workers' party. Both Katznelson and Kocka introduce an additional level between social and political class formation, one that encompasses a common social or cultural

[25] Ira Katznelson, "Working-Class Formation: Constructing Cases and Comparisons"; and Jürgen Kocka, "Problems of Working-Class Formation: The Early Years, 1800–1875," in Katznelson and Zolberg, *Working-Class Formation*, 3–41; 279–351.

[26] Hartmut Zwahr, *Zur Konstituierung des Proletariats als Klasse: Strukturuntersuchung über das Leipziger Proletariat während der Industriellen Revolution* (Berlin, 1978).

[27] Jürgen Kocka, *Lohnarbeit und Klassenbildung: Arbeiter und Arbeiterbewegung in Deutschland 1800–1875* (Berlin, 1983). See especially chap. 2, "Die Weberianische Verwendung eines Marxschen Klassenbegriffs," 23–30. Kocka's model of class formation is explicated in greater detail in his recent works, *Weder Stand noch Klasse: Unterschichten um 1800* and *Arbeitsverhältnisse und Arbeiterexistenzen*. Although only these first two volumes have appeared thus far, it seems likely that the four volumes will follow the four-tiered model of class formation outlined in *Lohnarbeit und Klassenbildung*. The forthcoming volumes are vol. 3, *Arbeiterleben und Protest: Entstehung einer sozialen Klasse*, and vol. 4, *Zwischen Volksbewegung und Klassenbewegung: Arbeiterorganisationen vom Vormärz bis 1875*.

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identity or the disposition to behave as a class. Kocka, for example, offers an interesting analysis of the "language of class," in particular of the emergence of the concepts of *Arbeiter* (worker) and *Arbeiterklasse* (working class) as positive terms of self-identification by the 1860s in Germany.[28] The fourth level encompasses class-conscious collective action that involves confrontation with other classes or the state and that, unlike Zwahr's third level, has no prescribed ideological content.

An additional important innovation in Kocka's model, as well as in Mary Nolan's contribution to the Katznelson and Zolberg volume, is the analysis of the role of the state in the process of working-class formation. Kocka points to the ways in which between 1800 and 1870 "governments of the larger [German] states facilitated the gradual intrusion of capitalist principles into the feudal structure of large-scale agriculture." At the same time, government policies toward trade and guilds "weaken[ed] traditional corporate identities" and helped journeymen "to identify as workers instead of as members of particular crafts or special skill groups."[29] Nolan argues in a similar vein that the newly unified German state "made workers a prime object of policy" in the wake of the depression of 1873. In placing workers "at the center of political debate" about the social question, state policy, "in both its repressive and paternalistic forms . . . promoted not only working-class political opposition, but the very process of working-class formation."[30]

Finally, in postulating an open-ended relationship among the four levels of working-class formation, in particular the absence of necessary stages or a natural progression from one level to the next, both Katznelson and Kocka disavow the teleology of Zwahr's model.[31] However, critics of the Katznelson and Zolberg volume have noted a lingering teleology in the revised levels approach. First, capitalist industrial development follows a unilinear path in this schema, and the notion of proletarianization appears as a starting point instead of a historical process to be analyzed. The structure of production is equated with economics, while the state, culture, ideology, gender, law, and religion are viewed as intervening variables rather than constitutive elements of production.[32] Moreover, economic structures figure as the motor of this model in that human agency and collective action are viewed as mere responses to material and social circumstances. Kocka, for example, points to the ways in which "the opportunities or risks for . . . [wage] workers are determined by markets and market changes . . . Their work is determined by those who possess all this in the form of capital and who, on this basis, employ and direct them."[33] Politics and the activities of

[28] Kocka, "Problems of Working-Class Formation," 327.

[29] Kocka, "Problems of Working-Class Formation," 289–91, 311.

[30] Mary Nolan, "Economic Crisis, State Policy, and Working-Class Formation in Germany, 1870–1900," in Katznelson and Zolberg, *Working-Class Formation*, 360–61.

[31] Kocka, *Lohnarbeit und Klassenbildung*, 27–29; and "Problems of Working-Class Formation," 283; Katznelson, "Constructing Cases and Comparisons," 17.

[32] Margaret R. Somers, "Workers of the World, Compare!" *Contemporary Sociology*, 18 (May 1989): 328–29; Joyce, "In Pursuit of Class," 172; Michael Sonenscher's review of *Working-Class Formation*, in *Social History*, 13 (October 1988): 386. See also John Breuilly, "The Making of the German Working Class," *Archiv für Sozialgeschichte*, 27 (1987): 546, 550, in which he reviews Kocka's *Lohnarbeit und Klassenbildung*.

[33] Kocka, "Problems of Working-Class Formation," 281–82; see also Somers' astute observations on this point; "Workers of the World," 328.

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human actors are displaced to levels three and four, and their importance in shaping economic or social structures is neglected.[34] Despite its firm rejection of teleology, Kocka's narrative conveys a strong sense of a progressive development toward class, one that is marked by the emergence of labor unions in the 1860s and the founding of the Social Democratic Party in 1875.[35]

Also problematic is Kocka's view that class formation advances when "non-class" lines of differentiation—ethnicity, nationality, religion, and gender—which run "next to, over, under or across class divisions" are relatively weak, while the relative strengthening of these "non-class" differentiations points to a process of class devolution.[36] In establishing dichotomies and oppositions between these various lines of social differentiation, he misses the contiguity of these differences in the formation of complex social identities. The implication of his argument is that working-class formation necessitates the relative weakening of other forms of identification. Again, this analysis of working-class formation presents a striking contrast to the literature on gender and middle-class formation, including Kocka's own observations on the German *Bürgertum*, which suggest that sexual difference and its explicit articulation were constitutive factors thereof.[37] Thus, however salutary the "shift from teleological to analytical" in these revised models of class formation, the relationships among the various levels of class, and between "non-class" and "class" distinctions, remain indefinite in Katznelson's and Kocka's models, a situation both authors hope to remedy by emphasizing the need for "empirical research." My historical case study of the relationships between gender and class among textile workers in Wilhelmine and Weimar Germany suggests, however, that the attempt to find a place for gender and women in this model of class formation disarranges teleologies, blurs boundaries between levels, or breaks apart the model altogether.

THE HISTORY OF THE FACTORY WORKPLACE, UNIONS, AND STRIKES in the textile industry of the Rhineland and Westphalia is rife with conflicts of gender and class. Gender was at the heart of the transition from home weaving and spinning to mechanized mills, as revealed in the dramatic campaigns of weavers across the Rhineland against "feminization" (*Verweiblichung*) and "displacement" (*Verdrängung*) during the 1880s and 1890s. The first Social Democratic and Catholic textile unions were founded under the shadow of this perceived social dislocation. Despite their divergent ideological traditions, the two organizations shared

[34] Somers, "Workers of the World," 328–29. Sonenscher refers to the analysis of politics in the essay collection as a study of "politics with much of the politics left out." See his review of *Working-Class Formation*, in *Social History*, 13 (October 1988): 387.

[35] Kocka, "Problems of Working-Class Formation," 349. In his *Weder Stand noch Klasse*, Kocka refers to the "Voranschreiten der Klassenbildung," 162.

[36] Kocka, *Lohnarbeit und Klassenbildung*, 29. I am using the term "non-class differentiations" for Kocka's term "nicht klassenmässige Trennungslinien." Kocka makes the same argument more forcefully in his *Weder Stand noch Klasse*, 38–39.

[37] Jürgen Kocka, "Vorwort," in Ute Frevert, ed., *Bürgerinnen und Bürger*, 9. In his estimation, "There are many indications that in addition to the class differences between the owners of the means of production and wage workers, sexual difference is one of the constitutive elements of the structural inequality of bourgeois society."

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A group of Crimmitschau textile workers on strike, January 1904. Deutscher Textilarbeiterverband, *Crimmitschau 1903–1928: Blätter der Erinnerung an Sachseus bedeutsamsten Arbeitskampf* (Berlin, 1929), 132. Reprinted courtesy of the archive of the Gewerkschaft Textil- Bekleidung, Düsseldorf.

perceptions of the changing world of industrial relations, of feminization and male displacement. Even if these perceptions and experiences were mediated differently, one in the language of class and the other in the language of estate and religion, both were influenced primarily by gender, specifically by the male experience of industrialization. Gender remained a point of contention and conflict as the textile industry became the largest factory employer of women and as the two textile unions organized the largest female contingents within the Socialist and Christian union confederations. The starting point of my study is the emergence after 1890 of Social Democratic workers' organizations that defined themselves in terms of "class" against employers and the state. The Deutscher Textilarbeiterverband (German Textile Workers' Union, or DTAV) formed a part of that collective self-definition through its membership in the confederation of "free" trade unions and its close ties to the Social Democratic Party. I would argue that class, as the primary political identity postulated among German workers, had a singular kind of discursive power in Germany between 1890 and 1930, not least because of the predominance of the Social Democratic Party and its unions among German workers. Class as political rhetoric permeated the discourses of Social Democracy, Catholicism, and liberal Protestant social reform efforts and echoed from the outposts of rural union locals to provincial and national parliaments. Even those who opposed the self-conscious "working class" embraced its vocabulary in articulating that opposition.

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The definition of class in terms of gender, indeed, the fragmentation of class by gender, is evident throughout the history of the DTAV, from the point of its formation during the 1890s to turning points in the early twentieth century when "class" had to be reconstituted: during World War I, the revolution of 1918, and the demobilization of 1918–1919; during the crisis of inflation and unemployment of 1923–1924, and again during the final years of the Weimar Republic. One side of this story is the articulation of male class identities, particularly in times of crisis when men, threatened with unemployment or wage cuts, explained social dislocation in terms of female competition. Waves of "proletarian anti-feminism" prompted weavers' strikes against the hiring of female "wage-cutters" during the 1880s and 1890s and accompanied government campaigns against female "double-earners" to secure jobs for men during the acute crises of demobilization, inflation, and depression of the Weimar Republic.[38] The other side of the story—the ways in which women confronted and eventually (after World War I) appropriated the rhetoric of class—shows that the boundaries of class were seldom fixed, that class formations and the exclusions on which they were based were continually contested and transformed. Although this particular story uncovers women who engaged and challenged the union bureaucracy, it is important to emphasize two points: class had meaning for women who remained aloof from the organized labor movement, and gender is central to the history of class even where women do not enter traditional "male" political arenas. The active presence of women in the German Textile Workers' Union only makes this story of contested boundaries and meanings more compelling.

The first task in analyzing the role of gender in the economic formation of the German working class (level one) is to determine the quantitative and qualitative participation of women in the process of production, to explore the changing contours of the female work force between 1880 and 1930, and to ask how the steady expansion of the female labor market might have influenced the relationships between men and women in the process of class formation.[39] Second, it is necessary to confront the texts and subtexts of labor historiography regarding women's work. The fact that women occupied a central place in the ranks of the first industrial workers across Europe raises questions about why women workers who appear in the history of factories and workshops (at level one) are invisible in histories of the labor movement and collective action (levels three and four).[40]

[38] "Proletarian anti-feminism" is a term from Werner Thönnessen, *Frauenemanzipation: Politik und Literatur der deutschen Sozialdemokratie zur Frauenbewegung 1863–1933* (Frankfurt, 1976), 5–7, 84, 147. See also Mary Nolan's case study, "Proletarischer Anti-Feminismus, dargestellt am Beispiel der SPD-Ortsgruppe Düsseldorf, 1890–1914," in *Frauen und Wissenschaft, Beiträge zur Berliner Sommeruniversität* (Berlin, 1976), 356–77.

[39] It is important to analyze the entry of women into new industries, as well as the "feminization" of already established industrial sectors. Important studies of sexual segregation in the German industrial labor market include Stefan Bajohr, *Die Hälfte der Fabrik: Geschichte d. Frauenarbeit in Deutschland 1914 bis 1945* (Marburg, 1979); Walter Müller, Angelika Willms, and Johann Handl, eds., *Strukturwandel der Frauenarbeit 1880–1980* (Frankfurt, 1983); and R. Stockmann, "Gewerbliche Frauenarbeit in Deutschland 1875–1980," *Geschichte und Gesellschaft*, 11 (1985): 447–75.

[40] The highly politicized *canuts* of Lyon, for example, were some 70 percent female. See Robert J. Bezucha, *The Lyon Uprising of 1834: Social and Political Conflict in the Early July Monarchy* (Cambridge, Mass., 1974); Strumingher, *Women and the Making of the Working Class*; Ivy Pinchbeck, *Women Workers and the Industrial Revolution, 1750–1850* (London, 1930, 1981). On the invisibility of women at levels

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Overcoming this invisibility in German labor history requires close attention to the gendered narratives of labor movement sources and to their uncritical assimilation by historians. For German labor history has accepted and reproduced a working-class ideal type, one that filled the pages of the union press and shaped the union's public representation and appeal. This ideal type was male and Protestant; his identification with job or craft—a prerequisite for the development of class consciousness—was based on skill acquired through formal apprenticeship training, relatively well-paid employment in an economically vital and highly productive industry, and long-term job stability. Implicit in the construction of this male ideal type was its opposite, the typical female factory worker: unskilled, "willig und billig" (submissive and cheap), and employed on a temporary or irregular basis. The canon of German labor history implies that women workers failed to appear at levels three or four because of the ways in which they participated in production: their youth, lack of formal skill training, and highly fluctuating work patterns precluded the formation of work identities.[41] German labor historiography is characterized by two underlying and contradictory assumptions regarding the relationship between production and class formation. First, the structure of production has a pivotal role in shaping the ways in which male workers viewed or related to their work and formed work identities.[42] At the heart of this relationship between worker and work was the possession of skill—real or mythologized—and the claim to comprehend the labor process as a whole or to assert some measure of control over that process. Political or class consciousness originated in the identities workers formed in the workplace, in particular in their struggles at the point of production. The second and contradictory assumption is that women's work identities, unlike men's, were not shaped primarily by their experience in and relationship to production. Marriage and

three and four, see Alexander, "Women, Class and Sexual Differences"; Dorothy Thompson, "Women and Nineteenth Century Radical Politics: A Lost Dimension," in Juliet Mitchell and Ann Oakley, eds., *The Rights and Wrongs of Women* (Harmondsworth, Eng., 1976), 112–38; Taylor, *Eve and the New Jerusalem*; Joan Scott, "Women in *The Making of the English Working Class*," in Scott, *Gender and the Politics of History*, 68–90.

[41] Leading German social historians, such as Gerhard A. Ritter and Klaus Tenfelde, claim that these attributes of female employment made the presence of large numbers of women in an industry "organisationserschwerend" (detrimental to unionization). See their essay, "Der Durchbruch der Freien Gewerkschaften Deutschlands zur Massenbewegung im letzten Viertel des 19. Jahrhunderts," in Heinz O. Vetter, ed., *Vom Sozialistengesetz zur Mitbestimmung, Zum 100. Geburtstag von Hans Böckler* (Cologne, 1975), 101–02. See also Karl Ditt, *Industrialisierung, Arbeiterschaft und Arbeiterbewegung in Bielefeld 1850–1914* (Dortmund, 1982), 236, who makes a similar argument. Brian Peterson elaborates the reasons why working-class women were unlikely to become politically active, even during the politically charged Weimar period. In his view, young working-class women were apolitical "because of the primacy of the personal" and older women because of the "sheer weight of the double burden of maintaining a household and working." Brian Peterson, "The Politics of Working-Class Women in the Weimar Republic," *Central European History*, 9 (1977): 99. Interestingly, these claims have scarcely been challenged during the ten or more years since they were made.

[42] Work identity is not a term that the nineteenth or twentieth-century labor movement actually used. Rather, I employ it to denote *Berufsinteresse* or *Berufsidentität*, which figures in union sources as a prerequisite to union membership. See, for example, Zentralverband christlicher Textilarbeiter Deutschlands (hereafter, ZCTD), "Warum braucht die Frau die Erziehung zur Gewerbetätigkeit?" *Textilarbeiterzeitung*, 12 (January 22, 1910): 10. The term is also implicit in more recent German labor historiography.

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motherhood, not the ten to twelve hours spent on the shop floor, are viewed as constitutive of women's work identities and political behavior.[43]

This view has been reinforced by a "new orthodoxy" in feminist history that has also sought to explain working women's identities and political activities primarily in terms of family values, thus effectively denying the authenticity of women's work experience. While feminist historians have dissolved the boundaries between the private and the public (or political) by uncovering the political fabric of the "private sphere," of consumption, reproduction, and neighborhood networks, some appear to have concluded that "the private" is the main or the only female political arena.[44] This tendency to confine women workers behind the lines of home and family, to read their activities in the arenas of factory, unions, and strikes as originating in and oriented primarily toward the family, underscores the tacit assumptions of German labor historiography that production was mainly a male arena and that neither gender nor women had a significant role in the vibrant political world of the German working classes.[45]

A close examination of textile personnel records challenges the image of the female mill employees as temporary, uncommitted workers. An analysis of male and female career patterns demonstrates that job stability in the textile industry was not a preserve of male workers, nor was instability inherently female. In fact, the yearly turnover rates for male textile workers were higher or nearly identical to those for women and men, and women had similar rates of stability and turnover when they worked in the same shops (weaving, spinning) at a similar level of skill.[46] Women's work patterns did differ from those of men in one important respect: the intersections of "family time" (particularly childbirth) and

[43] See, for example, Nolan, "Economic Crisis," 377.

[44] Eleanor Gordon refers to this "new orthodoxy" in her article, "Women, Work and Collective Action: Dundee Jute Workers, 1870–1906," *Journal of Social History*, 20 (1987): 27–28, 44. On the broader theme of the family or the private sphere as a locus of politics, see "Politics and Culture in Women's History: A Symposium," *Feminist Studies*, 6 (Spring 1980): 26–63. For a more specific examination of this theme for the working-class family, see Ellen Ross, "Fierce Questions and Taunts: Married Life in Working Class London 1870–1914," *Feminist Studies*, 8 (Fall 1982): 575–602; Temma Kaplan, "Female Consciousness and Collective Action: The Case of Barcelona, 1910–1918," in Nannerl O. Keohane, Michelle Z. Rosaldo, and Barbara C. Gelpi, eds., *Feminist Theory: A Critique of Ideology* (Chicago, 1982), 55–76.

[45] Some of the classic works in women's history, like Joan W. Scott and Louise A. Tilly, *Women, Work, and Family* (New York, 1978); Louise Tilly, "The Family Wage Economy of a French Textile City: Roubaix 1872–1906," *Journal of Family History*, 4 (1979): 381–93; and Alice Kessler-Harris, *Out to Work: A History of Wage-Earning Women in the United States* (New York, 1982), analyze women's participation in the waged labor force as part of "family strategies" or point to the importance of the family in shaping women's union behavior. Additional studies that propose interesting ways of thinking about this problem include Robyn Dasey, "Women's Work and the Family: Women Garment Workers in Berlin and Hamburg before the First World War," in Richard J. Evans and W. R. Lee, eds., *The German Family: Essays on the Social History of the Family in Nineteenth and Twentieth-Century Germany* (London, 1981), 221–55; and Jean Quataert, "Teamwork in Saxon Homeweaving Families in the 19th Century: A Preliminary Investigation into the Issue of Gender Work Roles," in Ruth-Ellen B. Joeres and Mary Jo Maynes, eds., *German Women in the Eighteenth and Nineteenth Centuries: A Social and Literary History* (Bloomington, Ind., 1986), 3–23.

[46] The personnel records include payroll or wage books and *Arbeiterstammrollen*, in which employers registered vital information on each worker, from eight textile mills in the Rhineland and Westphalia. My total valid sample at present includes approximately 3,600 workers. See Kathleen Canning, "Class, Gender, and Working-Class Politics: The Case of the German Textile Industry, 1890–1933" (Ph.D. Dissertation, Johns Hopkins University, 1988), chap. 3, "The Textile Workforce: Career Patterns and Work Identities."

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"industrial time" necessitated more frequent interruptions of their employment.[47] Contrary to common assumption, female textile workers were more likely to take a temporary leave from work upon marriage or the birth of a first or second child than to give up their jobs entirely. As the numbers of married women workers employed in textile factories increased steadily between 1882 and 1914, married women and single mothers established their own rhythms of work, interrupting employment to give birth, returning to work for a few months, leaving work again to care for a sick child or relative, and returning once again when the family situation permitted. As early as 1874, the factory inspector in one textile town noted that "women who are responsible for a household come to work later and leave work earlier [than the others]. When someone in the family is ill, they do not come to work at all. There is a general, unspoken agreement by which married women are treated with lenience. Their situation is therefore a relatively favorable one."[48]

Records from the Mechanische Weberei in Bielefeld confirm that women commonly returned to the mill after childbirth: over one-third of the women who left their jobs because of pregnancy returned to work within a few months after giving birth.[49] The profile of one woman who undoubtedly belonged to the *Stammarbeiterschaft*, or "core" of the textile work force, illustrates how women workers negotiated the demands of marriage, motherhood, and factory employment. Sofia Budde, weaver, worked at the Weberei for a total of twenty-nine years between 1890 and 1925. Hired as a weaver in 1890 at the age of fourteen, Sofia left her job for the first time in August of 1894 because of pregnancy. She returned to the mill two months later, presumably after the birth of her first child, and listed her new (married) name as Sofia Schneider. Two years later, she took a seven-month leave, again because of pregnancy. When Sofia returned to her loom in July of 1897, she was twenty-one and the mother of two small children. She stayed on the job until September 1905, when she interrupted her employment for nine months. This time, the reason for her departure was listed as "auf Wunsch" (at her own request), but in view of her lengthy absence, it is likely that she gave birth to another child during these months. Sofia returned to the Weberei in May 1906 and worked the next eleven years without interruption. Her last absence, between 1917 and 1921, was probably necessitated by the disruptions of textile production or changes in her family situation during and after the war. She returned to work again in 1921 and departed from the Mechanische Weberei for the last time in 1925 at the age of forty-nine.[50]

The pattern of Sofia Budde's factory career, as well as the general patterns outlined above, point to the ways in which women continually mediated the demands of family time and industrial time, shaping their own working lives as they interacted with structures (labor markets) and processes (industrial growth,

[47] Tamara Hareven, *Family Time and Industrial Time: The Relationship between the Family and Work in a New England Industrial Community* (Cambridge, 1982).

[48] Hauptstaatsarchiv Düsseldorf (hereafter, HStAD), Landratsamt Mönchen-Gladbach 710, Bericht des Gewerbeaufsichtsbeamten Mönchen-Gladbachs (December 14, 1874), 104–05.

[49] Stadtarchiv Bielefeld, Personalbücher der Mechanischen Weberei, 28/1, 29/1. This calculation was made for the entire period covered by the *Arbeiterstammrollen*: 1865–1924.

[50] Stadtarchiv Bielefeld, Personalbücher der Mechanischen Weberei, 28/1, 29/1.

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From a brochure published by the Social Democratic textile union in 1925 on pregnancy and women's work in the mills. DTAV, *Erwerbsarbeit, Schwangerschaft, Frauenleid* (Berlin, 1925). Reprinted courtesy of the Johann Sassenbach Stiftung/Zentrales Gewerkschaftarchiv, Berlin.

feminization). At the same time, mill owners sanctioned and facilitated these flexible rhythms of work. On the one hand, employers were dependent on married women workers, especially during periods of rapid industrial growth and acute labor shortages, which occurred frequently between 1890 and 1914. On the other hand, these gender-specific work patterns served as one explanation for women's lower wages and subordinate status as a "secondary" work force. These career patterns underscore the importance of expanding definitions of job stability to include those who remained "core workers" despite interruptions in their employment. My conclusion that many women workers were long-term and

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loyal employees offers no substantive evidence of the kinds of work identities they might have formed, but it does argue against the claim that the brevity of the average female factory career prevented women from developing work identities at all.

Finally, an analysis of level one or economic formation should emphasize the role of non-economic factors, ideological conceptions of gender roles and proper work for men and women, in shaping the structure of textile production—the deployment of textile machinery, the division of labor, and definitions of skill, work time, and wages—especially as its female work force expanded between 1890 and 1914. The social reformers, factory inspectors, entrepreneurs, and labor movement activists who participated in the intensive debates about female factory labor during this period redefined the sexual division of labor as they sought to resolve the growing discrepancy between the continued expansion of the female work force and traditional notions about the character of the sexes. Legislators and factory inspectors, for example, legitimated women's factory employment while attempting to contain it within strict boundaries. They sought to preserve a delicate balance between women's work and family duties, to regulate the work world in order to preserve the family, as the restrictions on women's work time, enacted in 1891 and again in 1908, make clear.[51] Textile employers, for their part, adhered to the prevalent norms of women's work in creating a female work force on which their industry was dependent but which was "secondary" in terms of wages and skill. At the same time, however, they challenged prevailing notions of sexual difference by actively recruiting married women workers and by rewarding them for long-term employment. Mill owners responded to the moral outcry against female factory labor with an array of paternalist "welfare" measures, such as cooking, sewing, and housekeeping classes, factory kindergardens, and company dormitories, measures that aimed to import the home into the factory, to make factory work compatible with the ideology of domesticity.

Male labor leaders and union members inserted their own fears of social and sexual disorder, of the dissolution of family, femininity, and masculinity into their campaigns for work-time reductions and wage increases. Their visions of domesticity were expressed in the image and rhetoric of the male breadwinner that marked these campaigns.[52] Even if most women workers took jobs out of economic necessity, many of them spurned the ideology of women's work that sought to restrict wage labor in favor of motherhood and housework. They came

[51] The labor code of 1891 excluded women from night work, limited women's regular work day to eleven hours, and extended pregnancy leave to four weeks. It also curtailed their hours on Saturdays and allowed women with families to leave work one half-hour early at lunchtime in order to give them adequate time to shop, cook, and clean. HStAD, *Jahresberichte der Königlich Preussischen Gewerberäte* (1892), 328–30 (hereafter, HStAD, *Jahresberichte*); Reichsamt des Innern, *Die Arbeitszeit der Fabrikarbeiterinnen, nach Berichten der Gewerbeaufsichtsbeamten* (Berlin, 1905). In 1908, the state again amended the industrial labor code, restricting women's work time to ten hours maximum during the first five days of the week and eight hours on Saturday; HStAD, *Jahresberichte* (1910), 420.

[52] One example is in Deutscher Textilarbeiter Verband (hereafter, DTAV), "Wann werden wir den freien Sonnabendnachmittag haben?" *Der Textilarbeiter*, 25–34 (August 15, 1913): 266–67. The Social Democratic Textile Workers' Union formulated its arguments in favor of Saturday restrictions in terms of women's domestic responsibilities.

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face to face with this ideology each time they transgressed gender boundaries: when they accepted jobs in "men's shops" or launched wildcat strikes without the support of male colleagues or union leaders. Women workers upheld their own visions of domesticity as they fought to imprint the factory regime with their needs as mothers and wives, raising frequent demands for longer lunch breaks, shorter working days, and better health and safety protection. Their visions often collided with those of male workers and employers: women resisted the representation of woman worker as mother, for example, when it was invoked to justify the discrepancy between male and female wages.

This brief explication of the ideological underpinnings of the structure of production illustrates the difficulty in extricating "economic formation" from politics, from conflicts between state and industry, mill owners and workers, women and men. Ultimately, it casts doubt on the efficacy of the levels approach and points to the fragility or artificiality of the divisions among the various levels.

LEVEL TWO OR THE SOCIAL FORMATION OF CLASS refers to webs of social relations that formed around common interests and experiences at work and in the realms of family and residential community. In each case, social class formation acts as a site of social interaction, of communication about the ways class is experienced in its economic and social dimensions; it is a site where common interests are identified, if not yet acted on. Hartmut Zwahr defines social class formation in terms of the increasingly complex networks of family, marital, social, and community ties that bound workers together within occupational groups as well as across traditional barriers of skill.[53] Kocka and Katznelson both seem to suggest that common interests arising from shared experiences in the workplace are essential elements of social class formation. Kocka, for example, emphasizes the importance of "a joint socio-economic position" on which "structural presuppositions of common interests" were formed, while Katznelson argues that social class formation was "determined in part by the structure of capitalist development," by workplace social relations and labor markets.[54] Zwahr proposes a different view, one that postulates a more flexible relationship between economic and social formation. His analysis of level one, the sphere of production, considers the diversity of experience, the barriers of skill, wage, and status that had to be overcome in order for class formation to advance. Social formation takes place as those barriers are crossed and different experiences transcended, as workers marry or choose godparents for their children across skill levels, as they congregate in urban neighborhoods, forging a common class identity that is not exclusively determined by production.[55]

The family is central in each of these versions of social class formation, although it figures in several different, often contradictory, ways. The family served as an agent of transmission by which one generation passed wage labor status and class consciousness on to the next. Zwahr's notion of a "born proletariat" is illustrative

[53] See Zwahr, *Zur Konstituierung*, 115–89, esp. 163–89.
[54] Katznelson, "Constructing Cases," 16; Kocka, "Problems of Working-Class Formation," 282.
[55] Zwahr, *Zur Konstituierung*, 163–89.

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of this point, for it distinguishes between the first generation of workers, who were born into the proletariat, and their fathers, who had been born into artisanal families and had suffered the dislocation and distress of proletarianization. In Leipzig, the "born proletariat" formed the vanguard of the emergent working class; it was the first to embrace Marxism and to establish class-conscious organizations. Thus, in Zwahr's schema, the family is viewed as a locus of transmission of class identity, a site of socialization and politicization.

While Kocka and Katznelson might agree that the family serves to transmit class culture and consciousness, their emphasis on the legendary "separation of home and work" as an essential aspect of class formation raises provocative questions about the enduring importance of family and household in the process. In the historical narratives of modernization, urbanization, and capitalist formation, the "separation of home and work" had political consequences in the division it effected between private and public. These narratives displaced politics onto the public realm, stripping family and household of their economic and political significance, of their "socializing and educative functions."[56] They also effaced the relations of power and dominance in families and households and rendered invisible the struggles within them over production, consumption, reproduction, and sexuality. The emphasis on this separation erases the importance of wage labor within many "modern" working-class households, in which women took in sewing, washing, and ironing or provided boarding, lodging, or child care. Furthermore, in making the home/workplace dichotomy an essential and universal marker of class formation, Kocka and Katznelson obscure the specifically gendered meaning of this purported separation "between work and non-work."[57]

Considering the prominence of family, household, and community in these innovative approaches to class formation, it is indeed remarkable that gender and women remain invisible. While Zwahr's focus is the Leipzig metalworking industry, in which only negligible numbers of women worked during the nineteenth century, he also overlooks women in his analysis of families and communities.[58] Women are perplexingly absent in his discussion of the born proletariat, even though they performed the reproductive labor of class formation: marrying working-class men, giving birth to "born proletarians," and nurturing the development of class consciousness in their children, as they shaped and passed on class traditions, cultures, and self-definitions. Moreover, many

[56] Richard J. Evans, "Politics and the Family: Social Democracy and the Working-Class Family in Theory and Practice," in Evans and Lee, *German Family*, 256. For examples of this narrative, see Kocka, "Problems of Working-Class Formation," 317–19; *Weder Stand noch Klasse*, 16; *Arbeitsverhältnisse*, 143, 478, 517; Katznelson, "Constructing Cases," 16.

[57] Kocka, "Problems of Working-Class Formation," 319. Certainly this difference "became more systematic and pronounced" for male workers, while few working-class women were able to enjoy a sphere of "non-work." Kocka points out that most wives of workers contributed something to the family income (as servants, washing women, part-time domestic workers, petty traders) yet continues to emphasize the importance of the "separation of home and work" (pp. 317–19). Jean Quataert's work, by contrast, underscores the political and economic significance of the household. See in particular Quataert, "Teamwork."

[58] The same can be said for Kocka's examination of marriage patterns, in which gender plays an obviously important role. Jürgen Kocka, "Family and Class Formation: Intergenerational Mobility and Marriage Patterns in Nineteenth-Century Westphalian Towns," *Journal of Social History*, 17 (Spring 1984): 411–33.

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women not themselves employed outside the home took an active role not only in the proverbial food riots but also in strikes and other forms of protest that involved members of their families or communities.

The authors of the "levels approach" to class formation assume that the shared interests of family and community superseded the divisions within them and, similarly, that shared class identities transcended the fissures of gender within family and household and those of ethnicity, religion, and gender within residential communities. The persistence and prevalence of "non-class" distinctions within families and communities raise fundamental questions about social cohesion as the outcome of social class formation: how did the experience of "class" differ between husbands and wives, sons and daughters, between generations within families; between Catholics and Protestants, Poles and Germans, between residents of industrial cities and of rural mill villages? These questions call for a more complex view of family, household, and community, one that considers the ways in which economic and social class formation are intertwined in these realms, explores the gendered fabric of household and community, and imparts to gender a decisive role in class formation, even where women did not participate in production to the same extent or in the same ways that men did. This is an endeavor that promises to dissolve the divisions between the four levels of class formation.

THE THIRD LEVEL OF CLASS FORMATION as defined by Kocka and Katznelson is also the most elusive. Kocka envisions the development of a common social identity grounded in the awareness or consciousness of a shared socioeconomic position. This social identity encompasses "some degree of internal cohesion and mutual communication, common experiences and dispositions, common fears and aspirations, manifest interests and loyalties, something like a common consciousness as a class—distinguished from the members of other classes."[59] Katznelson's definition of this level in terms of "shared dispositions" is similar. In his schema, shared dispositions are not necessarily "class dispositions"; they can also denote shared understandings of the social system or shared values of justice and goodness. Thus this level has an obvious cultural dimension in that it represents the ways in which "people construct meaning to make their way through the experienced world" or "the cultural configurations within which people act."[60]

The unbound configuration of level three can perhaps be made concrete by analyzing one kind of shared disposition: work identities, or the ways in which men and women related to and located themselves within the sphere of their waged work, a domain that encompassed their machines, the products of their labor, social networks on the shop floor, and even the physical space of the mill. Although work identities are admittedly difficult to measure or document empirically, it is possible to read a variety of sources "against the grain"—factory inspectors' reports, police and newspaper accounts of unrest, personnel records,

[59] Kocka, "Problems of Working-Class Formation," 282.
[60] Katznelson, "Constructing Cases," 18–19.

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and female social reformers' observations of mill life—in order to explore the ways in which workers viewed and utilized their jobs as expressed both in the rhythms of their mill careers and in their work ethics. Other dimensions of work identities include individual and collective self-definitions, through which workers both resisted and sought accommodation with the factory regime and the dominant ideology of gender roles that underlay it. Central to this inquiry is the assumption that work identities were embedded in, but not determined by, the structure of production. For integral to the concept of work identity is the possibility of agency, revealed in the ways in which workers sought to position themselves within and sometimes to distance themselves from the constraints of the structure of production.[61] The significance of the workplace in this process of identification does not imply the existence of a firm boundary between shop floor experience and family or community. Instead, the concept of work identities includes an exploration of the way identities were shaped by the continual intersections of family time and industrial time.

Of pivotal importance in imagining how women formed work identities and developed a sense of *Berufsethos* are questions about how they viewed and utilized their jobs, about why women went to work and remained at work in the textile mills. In most cases, it was not an abstract work ethic or loyalty to the loom that drove women (single, married, or widowed) into the factory. Regardless of the pride textile workers may have found in the products of their labor, most women worked out of dire economic necessity, as did most men.[62] However, the mill job also represented for women with families a complex network of support that extended beyond the monetary value of the wages they earned. The camaraderie and sociability of the shop floor and the solidarity of sharing the experiences of pregnancy, birth, illness, or widowhood were an important part of women's work experience. Observers noted that the gossip and chatter of the shop floor often drew women back to work after marriage or the birth of a child. Strike reports also offer insight into the significance of workplace friendships and loyalties in shaping work cultures.[63]

While most women worked out of economic necessity, those who worked for

[61] Agency, like class and experience, remains a point of contest between social historians who salute the "linguistic turn," and thus emphasize the ways in which agency was always "discursively constructed," and those who continue to uphold a notion of agency that denotes the actions and interventions of human beings in the making of their own history. On this point, see Scott, "Evidence of Experience," 777–78, 792–93; Parveen Adams and Jeff Minson, "The 'Subject' of Feminism," in Parveen Adams and Elizabeth Cowie, eds., *The Woman in Question: M/f* (Boston, 1990), 91–93.

[62] Factory inspectors undertook an extensive study of married women's factory employment in 1899 that refuted the popular notion that most women worked not for the bare necessities but in order to attain the "extras," like clothes, tobacco, and alcohol; HStAD, *Jahresberichte* (1899), 509, 513, 604.

[63] One social reformer who conducted an academic study of textile workers in a Rhenish textile town pointed out that the female worker went back to the mills after marriage not out of necessity but because she "misse[d] the 'events' in the factory workroom, the company of others, to which she has been accustomed since she was thirteen or fourteen years old." Marie Bernays, *Auslese und Anpassung der Arbeiterschaft der geschlossenen Grossindustrie: Dargestellt an den Verhältnissen der "Gladbacher Spinnerei und Weberei A. G." zu München-Gladbach im Rheinland* (Leipzig, 1910), 200. Police reports also indicate that women workers launched strikes to express their solidarity with female colleagues who had been fired or penalized. HStAD, Regierung Düsseldorf, no. 24706, no. 24693: "Bericht der Polizeiverwaltung von 26.11.1900"; no. 24704: "Bericht der Polizeiverwaltung von 25.2.1910."

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several years at one job undoubtedly took pride in their work. Minna Wettstein-Adelt, a social reformer who disguised her identity and went to work in a textile mill, offered insights into the work ethic among women weavers: "Many of these girls work enthusiastically, especially those who follow the completion of a whole piece, like those who weave smaller rugs or single fitted curtains. They love their machines, like one loves a loyal dog. They clean them until they shine and decorate them with colorful ribbons, holy cards, and other trinkets that their suitors won for them at last summer's fair."[64] The records of one charitable foundation, the Diergardt Stiftung of Viersen, also reveal that female textile workers expressed their pride in work through factory rituals, especially festive celebrations of employment anniversaries and awards for diligence. The female colleagues of Frau Schmitz, a reeler who received the Diergardt award on her twenty-fifth anniversary on the job, decorated her machine with flowers and ribbons, presented her with the gift of an armchair, and composed songs in her honor.[65] The centrality of the machine in these festive ceremonies offers testimony of the identification of women with their work and pride in the products of their labor.

Work identities can also be viewed as the location of individual and collective self-definition vis à vis employers, the state, and male workers. One manifestation of collective identities was the manner and style of workers' dress. Shop floor "fashions" signified women workers' awareness of their place in the process of production and in the moral regime of the mills. Female weavers reportedly dressed in a prim manner, which may have reflected their sense of belonging to the elite of female workers. Their distinct dress code may also be explained by the presence of large numbers of older, married women in the ranks who were often portrayed by employers as the custodians of the younger, more unruly women workers. Female spinners, by contrast, usually young and single, and considered to be the insolent and unruly force among female textile workers, often dressed in disorderly clothing yet manifested their own chic style.[66] It is likely that the high temperatures and dampness of the spinning shops had something to do with female spinners' "disorderly" dress. But the stylish character of that disorder may also have expressed a rebellion against the moral claims of the factory regime. The clothing that female spinners and weavers wore expressed their sense of belonging to a class of factory workers and distinguished them from other social classes.

A story from 1905 of one young woman who inadvertently violated the dress code illustrates this point: the young woman was Danish, and according to her country's custom, she wore a hat on the way to and from work and blouses with long sleeves while at work, both emblems of "respectability" among middle-class girls. Her appearance caused a sensation on the streets and in the mill, distracted several female workers from their jobs, and provoked demands from several of

[64] Minna Wettstein-Adelt, $3\frac{1}{2}$ *Monate Fabrikarbeiterin* (Berlin, 1893), 20.

[65] Stadtarchiv Viersen, Diergardt Stiftung III/056, 1904–1912: "Ehrungen und Belohnungen für Fabrikarbeiter aus der zu Mönchengladbach bestehenden Diergardt Stiftung, 1904–1912": Brief von C. H. Goeters an den Bürgermeister Stern von Viersen (April 4, 1905).

[66] Marie Bernays, "Berufsschicksale moderner Industriearbeiter," Part 2, *Die Frau*, 18 (December 1910): 212–15.

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them that she remove the hat or leave the workplace. The *Obermeister* averted a walk-out by promptly firing the young woman.[67] While enforcing their own rigid, class-based code of dress in the mills, female textile workers on Sundays frequently donned their prettiest dresses and did not disdain to wear hats, gloves, and jewelry or to carry parasols. The discrepant dress codes of work days and holidays underscores the significance of shop floor "fashion" in female work culture and in the formation of social class identity.

Women workers also developed their own moral code in response to employers' attempts to regulate morality and virtue through institutions of tutelage. A central component of workplace culture, this moral code allowed for both accommodation and resistance to the factory regime. Female mill workers expressed and enforced their own sexual mores at work, especially when they worked in predominantly female shops. Beyond the frankness about sexual experiences and the acceptance of out-of-wedlock pregnancies, recounted by middle-class observers, women workers also delimited their space in the mill by sexual horseplay, including the initiation of newcomers in sexual games and gestures. Wettstein-Adelt, for example, described her shock at the obscene behavior of her female co-workers, one of whom frequently expressed anger or displeasure by lifting her skirts and pulling down her underwear.[68]

Women workers also asserted and defended their own definitions of sexuality in their individual and collective protest against rape or molestation by managers and supervisors, apparently a common enough feature of shop floor life. This resistance took various forms, ranging from reporting the rape to the factory inspector or the union to collecting stories and "passing the word" through the mill in an attempt to protect female colleagues from subsequent assaults. Women frequently engaged in collective action to enforce and defend their own code of morality. In 1905, male and female workers in a Bocholt mill went on strike to demand the firing of an overseer who had raped several female employees.[69] The issue of rape led to a turbulent confrontation in the government district of Düsseldorf in 1902 when an angry crowd of both male and female workers assembled in front of the home of a master weaver who had assaulted a female subordinate. One worker was killed in the resulting melee, and four others received jail sentences.[70]

My analysis of work identities focuses on the multiple meanings women and men derived from their experiences of work. It coincides in many respects with Katznelson's analysis of cultural formation as "shared dispositions" or Kocka's notion of common fears, aspirations, interests, and loyalties. But I place gender at the center of my analysis of cultural class formation in order to illustrate that these meanings and identities were far more complex than a "consciousness of a shared

[67] DTAV, *Der Textilarbeiter*, 17/41 (October 13, 1905), local report from Mönchen-Gladbach.

[68] Wettstein-Adelt, $3\frac{1}{2}$ *Monate*, 31. Alf Lüdtke's work on *Eigensinn* has influenced my reading of my sources on dress code and sexuality. See in particular his essay, "Organizational Order."

[69] ZCTD, *Der christliche Textilarbeiter*, 7/27 (July 8, 1905), local report from Bocholt. The workers also raised the demand for higher wages in this strike. See also Wettstein-Adelt's accounts of sexual harassment in textile mills in $3\frac{1}{2}$ *Monate*, 28–29.

[70] For additional reports of sexual abuse and workers' demands to fire perpetrators, see HStAD, *Jahresberichte* (1902), 327; (1905), 265; (1912), 367–68.

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socio-economic position."[71] The world behind the mill gate was just as likely to be rent apart by gender divisions as it was to be welded together by class identification. My critique posits a flexible and historically specific notion of class, one that locates "cultural" identities of class not only in conflicts about wages or work time but also in confrontations about pride and honor, gossip and respectability, bodies and sexuality. It inserts into level three of the model of class formation experiences and meanings that had no necessary consequences for level four, the formation of workers into a political class. Indeed, labor leaders sought to expunge many of the experiences analyzed here from the realm of organized labor politics.

THE CULMINATION OF THE PROCESS OF CLASS FORMATION in each of the models discussed here is the political expression of a collective class identity (level four). Hartmut Zwahr views political class formation as the development of a class-conscious proletariat that has embraced a specific ideology, that of scientific Marxism, while Katznelson and Kocka focus on the organizational form, rather than on its ideological content. A political class is one that is "organized and that act[s] through movements and organizations to affect society and the position of the class within it" (Katznelson) or one that "may act collectively and perhaps organize in conflict with other classes and perhaps the state" (Kocka).[72] Introducing gender into the level of political class formation requires, first, a critical engagement with the assumptions of German labor history regarding women workers' proclivity (or lack thereof) toward political activism. This inquiry is best located in a largely (but by no means exclusively) "female sphere" of industry such as textiles, where significant numbers of women worked, joined unions, and went on strike. In examining the encounters between women and the organized labor movement, I intend not only to document but also to explain the varying patterns of union membership and the divergent goals of male and female activists. I prefer to resist easy references to women's "nature" and seek instead to uncover the ways in which the politics of class might have inhibited the participation of women in the organized labor movement.

Female activists in the textile unions, reporting on their recruiting efforts among women workers, often recounted the difficulties in winning women's loyalties to the union. Many women, torn between the double burdens of work and care of family and household, were unable to find the time to devote to union meetings or organizing campaigns, as is poignantly illustrated by women workers' accounts of *Mein Arbeitstag—mein Wochenende* (My working day—My weekend), published by the DTAV in 1930.[73] Others could not afford the expense of dues, even if they were less expensive for women. Still others were constrained by familial authority structures, by the sanctions of fathers or husbands against

[71] Kocka, "Problems of Working-Class Formation," 282.
[72] Katznelson, "Constructing Cases," 20; and Kocka, "Problems of Working-Class Formation," 283.
[73] DTAV, *Mein Arbeitstag—mein Wochenende* (Berlin, 1930). A new edition has recently been published with an excellent introduction by Alf Lüdtke: *Mein Arbeitstag—mein Wochenende: Arbeiterinnen berichten von ihrem Alltag 1928* (Hamburg, 1991).

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political engagement on the part of their daughters or wives. A close reading of union newspapers and conference minutes reveals another barrier to women's participation in or long-term commitment to union politics: the "anti-feminist" sentiment of many male unionists, expressed occasionally in strikes against the hiring of female weavers, in recurrent resistance to the demand for equal pay for equal work, and in the persistent reluctance of union functionaries to grant women a permanent place in the union bureaucracy.[74] This "anti-feminism," juxtaposed with the DTAV's claim to represent all textile workers, made the union an intensely contradictory arena for the unorganized women workers whom the union sought to recruit and for the committed female union activists, who confronted this sentiment at every level of the union bureaucracy. On the one hand, the DTAV adhered to the Social Democratic Party's (SPD) view of waged work as an essential prerequisite of women's emancipation and frequently defended the right of women to paid employment on these grounds.[75] On the other, in both its internal discourse and its appeal to the public, the union created and upheld an image of the working woman as a "wage-cutter" or strikebreaker, a ruthless competitor, a volatile, unreliable ally or a passive, apathetic victim of capitalist exploitation. Female activists pointed to the powerful effects of this negative imagery on the political behavior of women workers, many of whom responded to these representations by remaining aloof from the organized labor movement or by articulating their demands in other forms such as spontaneous strikes.[76]

Despite the many possible reasons why women workers may not have been inclined to embrace a vocabulary of "class" or to express their concerns by participating in "class-conscious" organizations or actions, membership statistics reveal that significant numbers of women workers surmounted familial and social barriers and joined the DTAV. In 1897, a few years after the DTAV's first congress, some 2,400 women comprised a mere 11 percent of its membership. During the next decade, the union experienced a process of feminization that paralleled the transformation of the textile work force and by 1907, women represented 37 percent of DTAV rank and file. As the union continued to grow before World War I, this figure remained relatively constant (see graph). Between 1913 and 1919, female membership more than tripled, as women came to dominate the union during the war. It increased again dramatically between 1919

[74] Thönnessen, *Frauenemanzipation*; Nolan, "Proletarischer Anti-Feminismus." Martha Hoppe became the first paid female union secretary in 1908, but she was consistently denied the right to vote in the union's executive committee. DTAV, *Protokoll der 9. ordentlichen Generalversammlung, abgehalten 1908 in Leipzig*, 9. Union leaders continued to oppose integration of women into all levels of union administration until 1927, when Elsa Niwiera became director of the Women's Bureau and full-fledged member of the executive with the right to vote on all union affairs. See DTAV, *Protokoll des 14. Verbandstages, abgehalten 1921 in Breslau*, 90–91, 146, 215–18, 259–60; DTAV, *Protokoll des 16. Verbandstages, abgehalten 1927 in Hamburg*, 53, 108, 219; DTAV, *Jahrbuch 1927* (Berlin, 1928), 194–95.

[75] Thönnessen, *Frauenemanzipation*, 41–48; on the diverging views of women's emancipation between the "Lassalleans" and "Eisenachers" within the early SPD, see 13–16, 32–34.

[76] DTAV, *Protokoll der 10. Generalversammlung, abgehalten 1910 in Berlin*, 237, 241; DTAV, *Protokoll der 12. Generalversammlung, abgehalten 1914 in Dresden*, 113. Also see DTAV, "Warum sind die Frauen so schwer für die Gewerkschaft zu gewinnen?" *Der Textilarbeiter*, 20, no. 42 (October 16, 1908); and "Die Meinung einer Kollegin zur Arbeiterinnenfrage," *Der Textilarbeiter*, 22, no. 36 (September 9, 1910): 283.

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SOURCES: DTAV, *Jahrbuch 1927*, 147; *Jahrbuch 1932*, 65.

and 1923, when nearly one-half million women belonged to the DTAV.[77] As the table on page 761 illustrates, by 1925 the unionization rate for female textile workers had surpassed that of men: 30 percent of female textile workers, compared to 24.5 percent of their male colleagues, belonged to the DTAV. Furthermore, women went out on strike in numbers that equaled, and during the years 1919 to 1924 even exceeded, those of male textile workers.[78] What implications do these numbers have for analyzing the role of gender in the process of political class formation? Do they signify or attest to the acquisition of "class identities" on the part of female union members and strikers? If so, how did the experiences, identities, and languages of class differ between men and women, and in what ways did the divergence between them fracture the unity of the "organized working class"? In what sense can the moments of solidarity be related to the moments of conflicting interests between men and women? How did women position themselves within this class: did they make their own meanings, formulate their own agendas, stake a claim to their own space within the labor movement?

Women emerged in the DTAV with their own political vision only after 1908,

[77] DTAV, *Jahrbuch 1927*, 147.
[78] Canning, "Class, Gender, and Working-Class Politics," chap. 6, 357–65.

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Percentage of Unionized Textile Workers, German Empire

	1895	*1907*	*1925*
MALE EMPLOYEES in Textile Industry	517,230	529,008	514,858
Male Members in Textile Unions			
DTAV (German Textile Workers' Union)	n.a.	74,883	126,172
percent employed		14.2%	24.5%
ZCTD (Central Union of the Christian Textile Workers of Germany)	n.a.	29,288	32,986
percent employed		5.5%	6.4%
TOTAL (both unions)	n.a.	104,091	159,158
percent employed		19.7%	30.9%
FEMALE EMPLOYEES in Textile Industry	427,961	528,235	681,262
Female Members in Textile Unions			
DTAV	n.a.	43,250	204,504
percent employed		8.2%	30.0%
ZCTD	n.a.	12,628	45,585
percent employed		2.4%	6.7%
TOTAL (both unions)		55,878	250,089
percent employed		10.6%	36.7%

SOURCES: Deutscher Textilarbeiterverband (DTAV), *Jahrbuch 1927* (Berlin, 1928), 147; Zentralverband christlicher Textilarbeiter Deutschlands (ZCTD), *Jahrbuch 1932* (Düsseldorf, 1932), 66; Wilhelm Böhmert, "Wandlungen der deutschen Volkswirtschaft 1882–1907; Ergebnisse der Berufs- und Betriebszählungen," *Arbeiterfreund*, 48 (1910): 24–25, 136; and George Neuhaus, "Die berufliche und soziale Gliederung im Zeitalter des Kapitalismus," *Grundriss der Sozialökonomik*, vol. 9, 399, 424–25.

when the union appointed Martha Hoppe as its first female secretary and officially addressed the "woman question" at its congress that year.[79] Early on, female activists in the union sought to expose the disparity between the DTAV's official program and the practice of the male rank and file in the everyday life of the union. The activities of female unionists—who worked under Hoppe's leadership to fulfill the demands of women workers for a more equitable wage, a shorter working day, expanded health and safety protection, and equal representation in the union bureaucracy and press—fueled the anti-feminist sentiments of male members and union leaders. Indeed, the most significant encounters between male and female union activists took place on the terrain of imagery and rhetoric, as each group upheld its own vision of sexual difference and inscribed it with distinct political meanings.

At the center of these conflicts about equality and sexual difference in the DTAV was the contested notion of *weibliche Eigenart*, or the "particularity" of the female sex. In one sense, this term embodies the profound ambivalence of party and union regarding the relevance of sexual difference for the politics of class. For example, female SPD leaders and functionaries could assert at one moment that "proletarian men" and "proletarian women" had fundamentally "the same interests" and that the Social Democratic struggle sought to join "all of the

[79] DTAV, *Protokoll der 9. Generalversammlung 1908*, 9, 209; Canning, "Class, Gender, and Working-Class Politics," 329–32. The year 1908 also represents a turning point in the relationship between women and men in the DTAV because of the repeal of the Prussian Law of Association that had impeded women's participation in associations that could be viewed as "political."

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exploited, regardless of sex."[80] In the next moment, however, they recognized women's distinct experiences at work, the particularity of the double burden of waged work and housework or child rearing, women's political inexperience and precarious legal status, and issued an urgent call for separate institutions for women within the union. Interestingly, the socialist-feminist and middle-class feminist movements ascribed similar meanings to the term *Eigenart*, despite the open animosity between the two. Both proposed a politics of *Eigenart* based on women's particular existence, activity, sentiments, and consciousness, which were not lesser, merely different, than men's. As a political slogan in both movements, *Eigenart* refused the vision of a "seamless integration of women into male spheres of employment or politics" that would necessitate the loss of female identity.[81] Instead, the politics of *Eigenart* demanded a place for a specifically female identity in the spheres of formal and informal politics.

Male labor leaders also embraced a notion of female *Eigenart*, one that disparaged female particularity as an impediment to their organizing efforts. Male leaders of the DTAV, for example, employed two distinct, often contradictory, arguments against incorporating female *Eigenart* into the politics of class. When faced with a challenge from within, or with the prospect of restructuring the union on behalf of women, they vigorously denied women's special needs, asserting that equality between the sexes already existed in the union and offering as evidence only their own practice of refusing preferential treatment to one sex. But, when pressed to explain the varying patterns of men and women's union or strike participation, male unionists admitted that women had particular duties—housework and child rearing, for instance—that bound them to the home and a state of perpetual backwardness, which they did not propose to change. Thus, in the official union rhetoric, *Eigenart* signified passive, apolitical women workers who were, at best, a costly burden on the labor movement and who, in the worst instances, betrayed the union's struggle by acting as wage-cutters and strikebreakers.[82] Male union leaders implemented the rhetoric of *Eigenart* in order to deny women entry into, to define women out of, the realm of class. In their view, accommodating female *Eigenart* in the sense defined by female activists would

[80] Ottilie Baader, "Bericht der Sozialdemokratischer Frauen Deutschlands an die Internationale Konferenz Sozialistischer Frauen," in Heinz Niggemann, ed., *Frauenemanzipation und Sozialdemokratie* (Frankfurt, 1981), 128–30. See also discussions within the SPD of protective legislation for women workers: Karin Bauer, *Clara Zetkin und die proletarische Frauenbewegung* (Berlin, 1978), 125; Quataert, *Reluctant Feminists*, 39–45. Richard J. Evans, *Sozialdemokratie und Frauenemanzipation im deutschen Kaiserreich* (Berlin, 1979), 44–50, discusses this ambivalence in August Bebel's work *Die Frau und der Sozialismus* (first published in 1878).

[81] Irene Stoehr, "'Organisierte Mütterlichkeit': Zur Politik der deutschen Frauenbewegung um 1900," in Karin Hausen, ed., *Frauen suchen ihre Geschichte: Historische Studien zum 19. und 20. Jahrhundert* (Munich, 1983), 222–23, 228–29. For additional discussion of *Eigenart* in the middle-class women's movement, see Helene Lange, "Steht die Frauenbewegung am Ziel oder am Anfang?" *Die Frau*, 29 (November 1921): 33–46; and Barbara Greven-Aschoff, *Die bürgerliche Frauenbewegung in Deutschland, 1894–1933* (Göttingen, 1981), 37–43. Various Social-Democratic versions of *Eigenart* can be found in Käthe Duncker's contribution to the debate on the SPD's conference on women: "Autonomie oder Integration in die Partei? Debatte auf der Frauenkonferenz 1908," in Niggemann, *Frauenemanzipation*, 139; Lily Braun, *Die Frauenfrage: Ihre geschichtliche Entwicklung und wirtschaftliche Seite* (1901; rpt. edn. Berlin, 1979), 199–200, 207; Bauer, *Clara Zetkin*, 130.

[82] DTAV, *Protokoll der 10. Generalversammlung 1910*, 87, 120, 135–36; and *Protokoll der 12. Generalversammlung 1914*, 118. See also DTAV, "Für die Frauen, von den Frauen," *Der Textilarbeiter*, 21 (1909): 5.

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divide the labor movement and divert its leaders' attention from the union's principal goals. In the view of male leaders, the unions could afford neither. Instead, women were to shed their *Eigenart*, their gender, in order to join the working class.

Female union activists, by contrast, articulated a concept of *Eigenart* that acknowledged the simultaneity of work and family in shaping women's work identities. They perceived a political potential in these overlapping identities that was overlooked, often scorned, by men. Conceding that women workers lagged far behind men in terms of associational traditions, practical experience, political understanding, and self-confidence, women activists sought to create a space within the DTAV in which female *Eigenart* would be respected, in which it could become the basis not of female backwardness, as male leaders argued, but of political mobilization. The politics of *Eigenart* did not represent a rejection of the politics of class so much as a contestation of the boundaries established by male leaders. Women insisted, for example, not on separate organizations, rather on their own space within the union, a space in which the renunciation of gender was not a prerequisite of class. They urged the union to formulate its women's program in terms of *Eigenart* "until the transformation is complete, until our female colleagues feel that they have equal rights and equal duties."[83] Intrinsic to their political concept of *Eigenart* was a critique of the universalist claims of class and its focus on production, a refusal of the dichotomy between women's work for wages and their work as mothers and wives. In claiming special consideration of women workers as mothers and wives, female union activists challenged the dominant identity and ideology of class in the German Social Democratic labor movement.

Prior to World War I, especially between 1908 and 1914, female activists in the DTAV sought recognition of female *Eigenart* in their everyday struggles in the mills for higher wages and a shorter work day, for greater protection of pregnant workers, and through their campaigns within the union to hold separate women's meetings, to establish a women's supplement to the union paper and increase the numbers of women in the higher echelons of union administration. Although they made modest gains in these areas, women remained a minority in the union, and negative images of women workers continued to pervade union rhetoric. The discordance between these images and women's own identities may explain why many more women workers sought to redress their grievances in strikes rather than through union activity.[84] Despite the predominance of women in the textile mills and the growing numbers of women in the DTAV before the war, female activists' efforts to redefine the politics of "class," embedded in male notions of

[83] DTAV, *Protokoll der 10. Generalversammlung 1910*, 234–35.

[84] Strikes, rooted in the workplace community, may have represented a less contradictory political arena than unions, one in which women could more easily reconcile their identities as mothers, workers, and wives in their demands for higher wages, shorter working hours, and safer conditions of work. For example, during the years 1902 to 1904, women made up between 17 and 23 percent of DTAV rank and file but 53 percent of participants in so-called offensive strikes. Of male strikers, 78 percent were unionized, while 63 percent of striking women belonged to the DTAV. DTAV, *Protokoll der 7. ordentlichen Generalversammlung, abgehalten 1904 in Linden-Hannover*, 42–45. During 1906–1907, women again represented 53 percent of participants in offensive strikes but only 36 percent of DTAV members. DTAV, *Protokoll der 9. Generalversammlung 1908*, 40–43.

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work ethos, skill, and wage, remained largely unsuccessful. Yet they seeded the ground for a postwar transformation of union politics. On the eve of the war, one female organizer predicted that the growing presence of women would fundamentally change the face of the unions: "The future in our organization belongs to women . . . It is even possible that men will themselves one day have to fight for equality in our organization."[85]

The upheaval of war, revolution, and demobilization transformed the experiences of female factory workers, as well as the discourses about women's work. These moments of rupture are crucial in understanding the feminization of union politics that occurred during the mid-1920s in the DTAV. First, the boundaries between *Frauen-* and *Männerindustrien* (female and male industries) dissolved as thousands of male workers left for the front and women were transferred to previously male sectors of production. Many of those who remained in the textile mills, as well as those who sought work in the armaments plants, were trained to use and repair complex machinery, to perform "skilled" or supervisory jobs.[86] Then, as the wartime economy became increasingly dependent on women, the state, employers, factory inspectors, and local officials joined efforts to "enlighten women about how urgently their work is needed."[87] Women's work for the fatherland was no longer "secondary" or detrimental to the family, to men's wages, or to masculine work identities. It was imbued, if only for a few years, with the honor and esteem that had otherwise been reserved for skilled male labor.

A parallel process took place within the textile unions as thousands of women assumed leadership of local union posts in the absence of male functionaries. Within a few years, the DTAV became a predominantly female union: by 1916, women had come to comprise some 60 percent of members, and this figure increased to nearly 75 percent by 1918. The political rhetoric of the union changed markedly as punitive reprimands of female backwardness gave way to respectful acknowledgement of the fact that women had indeed become "the core of the organization."[88] Moreover, outside the unions, increasing numbers of female workers defied the *Burgfriede* (agreement between the unions and government for domestic peace during wartime). Women comprised 62 percent of industrial strike participants in 1916 and 75 percent in 1917.[89] In sum, the departure of so many men from their families, their jobs, and their union posts may have encouraged the development of a female sub-culture, a wartime

[85] DTAV, *Protokoll der 12. Generalversammlung 1914*, 113.

[86] HStAD, Regierung Düsseldorf, no. 33581, "Bericht der Gewerbeinspektor für Crefeld Stadt und Land und Kreis Kempen von 30.3.1917." According to this report, many women received specialized job training during the war, including some who were trained to maintain and repair textile machines. See also Ute Daniel, *Arbeiterfrauen in der Kriegsgesellschaft: Beruf, Familie und Politik im Ersten Weltkrieg* (Göttingen, 1989).

[87] HStAD, Regierung Düsseldorf, no. 33485, correspondence marked 174/8 1917. This correspondence is damaged, but it seems likely that it was compiled by a local *Kriegsamtstelle* in the Rhineland for the *Regierungspräsident* in Düsseldorf.

[88] DTAV, *Protokoll der 13. Generalversammlung, abgehalten 1917 in Augsburg*, 127–29. DTAV, *Jahrbuch 1914–1915* (Berlin, 1916), 282, 307; *Jahrbuch 1917* (Berlin, 1918), 72–74. The number of women who held union posts grew from some 1,800 in 1913 to 3,000 in 1917.

[89] DTAV, *Jahrbuch 1916* (Berlin, 1917), 95–97; *Jahrbuch 1917*, 63–71. Only 26 percent of those involved (male and female) in 1916 and only 36 percent in 1917 were union members.

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anomaly that allowed the politics of *Eigenart* to flourish, while at the same time women were proving that they could take on "men's jobs" and "men's responsibilities." The extent to which the war had drawn women workers into the arena of labor politics became evident during 1918, when female membership began to expand at unprecedented rates, reversing the ratio between men and women in some locals within a few months.

As women came to predominate in the DTAV, the politics of *Eigenart* acquired a new meaning, and female activists emerged in the union with a renewed energy. However, they faced new challenges and new forms of "anti-feminism" when the war came to a close and the government of Wilhelm II collapsed. In view of the tumultuous political and economic conditions at the close of the war, the new Social Democratic government sought not only to reorder the relations between labor and capital in a fundamental sense but also to alleviate the urgent problems of unemployment and unrest by recasting the sexual division of labor. The demobilization decrees aimed to return veterans to their jobs as quickly as possible and called for the immediate dismissal of women whose male family members could now earn a living. Demobilization marked a sudden and powerful transformation of the social climate surrounding women's work. Government and unions unleashed vigorous campaigns against the so-called *Doppelverdiener* (double-earners).[90] The juxtaposition of the rapid erosion of economic rights with the newly acquired right to vote, both of which were cloaked in the rhetoric of civic equality for the sexes, profoundly affected female textile workers' views of work and politics. Between December 1918 and the end of 1920, female membership in the DTAV increased by over 450 percent; some 260,000 women joined the union. Tens of thousands of women joined male colleagues and workers from other industries in militant political strikes that shut down the entire Rhineland and Ruhr between 1918 and 1920.

These experiences, in both their social and discursive dimensions, form the backdrop for the feminization of politics and the articulation of new political identities during the Weimar Republic. While the numbers of new female members skyrocketed, the union found itself embroiled in internal conflicts when union leadership and rank and file split between the radical or Independent Social Democratic Party (USPD) and Majority Social Democratic Party. Enmeshed in conflict and imbued with the new rhetoric of civic gender equality, the DTAV executive voted in 1919 to dismantle the Women's Bureau: its members had concluded "that a special type of training for women was not necessary."[91] Male union leaders, who had long ago mastered the rhetoric of equality, which rendered female *Eigenart* superfluous, even reactionary, now pointed to women's

[90] Richard Bessel, "'Eine nicht allzu grosse Beunruhigung des Arbeitsmarktes,' Frauenarbeit und Demobilmachung in Deutschland nach dem Ersten Weltkrieg," *Geschichte und Gesellschaft*, 9 (1983): 211–29; Susanne Rouette, "'Gleichberechtigung' ohne 'Recht auf Arbeit': Demobilmachung der Frauenarbeit nach dem Ersten Weltkrieg," in Christiane Eifert and Susanne Rouette, eds., *Unter allen Umständen: Frauengeschichte(n) in Berlin* (Berlin, 1986), 159–82; and Susanne Rouette, "Die sozialpolitische Regulierung der Frauenarbeit: Arbeitsmarkt- und Fürsorgepolitik in den Anfangsjahren der Weimarer Republik; Das Beispiel Berlin" (Phil. dissertation, Technische Universität Berlin, 1991).

[91] DTAV, *Protokoll des 14. Verbandstages, abgehalten 1921 in Breslau*, 146.

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experiences of war, revolution, and democracy as proof of their "equal" abilities and status.

When the postwar political crisis had subsided, DTAV leaders faced new challenges from the union's female members, now backed by the presence of many more active women. They reasserted their earlier demands for a female representative to the union's executive, disrupting union conferences during the early 1920s with their expressions of outrage about the union's refusal to allow secretary Martha Hoppe the right to vote. Moreover, the postwar politics of *Eigenart* insisted that the union address women's reproductive burdens—pregnancy, birth control, abortion, and housework. Between 1925 and 1928, the female body emerged in the arena of class politics when the DTAV investigated the conditions of pregnant women workers and presented its shocking findings to the Reichstag in 1925; when it organized demonstrations against Paragraph 218, the restrictive abortion law; and, finally, when it placed housework at the center of its women's program, sponsoring an essay contest for female textile workers on the relationship between waged work and housework in 1928 and publishing the collected essays in 1930.[92] The appearance of a politicized female body in the sphere of union politics stands in stark contrast to the pre-war politics of class. In forging a political link between the home and the workplace, the private body and the social and political bodies, female activists in the DTAV fostered the formation of a female political identity within the organized labor movement. In uncovering the political meanings of sexual difference that divided the labor movement, they resisted and transformed the ideology of class.

This brief overview of the relationship between gender and class in the DTAV portrays a union movement that had, at certain points in its history, taken on the form of political class spelled out in Katznelson and Kocka's fourth level. Central to the appropriation of the ideology and identity of class were the boundaries it drew around itself of religion, ethnicity, and gender. While exclusion and subordination were intrinsic to the process of class formation, the DTAV's gendered boundaries were seldom fixed. This is particularly clear in the case of the textile union, where the steady growth of female membership, the emergence of a core of female activists, and their articulation of female *Eigenart* periodically challenged and destabilized the content of union policies, the structure of union hierarchies, and ultimately the politics of class.

IN THIS DISCUSSION OF THE "LEVELS APPROACH," I have sought to uncover the presence of women and to argue for the relevance of gender at each level of class formation. Serious questions about the usefulness of this approach arise when it is viewed from the perspective of gender and subjected to empirical and historically specific examination. The lens of gender permits specific insights into

[92] DTAV, *Umfang der Frauenarbeit in der deutschen Textilindustrie: Erwerbsarbeit, Schwangerschaft, Frauenleid* (Berlin, 1925); Max Hirsch, *Die Gefahren der Frauenerwerbsarbeit für Schwangerschaft, Geburt, Wochenbett und Kindesaufzucht mit besonderer Berücksichtigung der Textilindustrie* (Berlin, 1925); DTAV, *Protokoll vom 1. Kongress der Textilarbeiterinnen Deutschlands, abgehalten am 11. und 12. Oktober 1926 in Gera*, 4–12; DTAV, *Mein Arbeitstag—mein Wochenende.*

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the arbitrary nature of the divisions among the four levels, as the discussions of economic and social class formation show. More specifically, the use of gender as a category of historical analysis dissolves the sense of progress that is implicit in the notion of levels, by revealing, for example, that the "dispositions or identities" purportedly formed at level three often contradict or challenge the goals and visions of the organized labor movement that represent level four. A consideration of the role of politics and ideology in shaping the process of economic class formation further diffuses this sense of progress among the various levels. Finally, the concept of gender resists the inherent tendency of this approach to reduce class politics to the singular dimension of waged work and uncovers the ways in which reproductive issues, as one example, transformed the ideology of class in the DTAV during the 1920s.

This critique appears to open an endless series of questions regarding the continued usefulness of our conceptual vocabulary, in particular the terms "class formation" and "class." One is whether a new and gendered view of class will bring about a Thompsonian revival of historical narratives that are "so complex, so permeated with social particularities and the subjective sentiments of the people in [the] story" that they cannot be reduced to abstract models.[93] In refraining from formulating alternative models that propose to integrate differences of gender, race, religion, or ethnicity, I conclude with a view of class that is "destabilized" yet historically specific. It calls for careful delineation between class as an analytical concept and class as a postulated identity or ideology, which is always embedded in specific historical settings. Thus "theorizing" class and class formation at this juncture might mean transcending the "social particularities" of class formations not through the establishment of new models but by linking or comparing, for example, different historical encounters between gender and class, or distinct ways in which populist or nationalist identities overlapped with those of class.

Attention to the historically specific ideologies and identities of class, approaching class "from the perspective not of teleology, but of . . . genealogy," requires neither the construction of a new model nor the revival of a historical narrative.[94] Rather, it demands a weaving of theory into historical research, a continual rethinking of our analytical categories grounded in a close reading of historical social vocabulary. In granting history an important place in the work of theoretical renovation, I have tried to move beyond the "epistemological crisis" engendered by the "linguistic turn" in history toward new visions of class and a historical language that embraces, rather than obscures, the conflictual meanings of class.

In emphasizing conflictual meanings, my study conceives of class as more than a "discursive construction." It also explores class as an identity and an ideology that was shaped as women and men assigned and contested the meanings of class. Furthermore, attending to this process of assigning and contesting meanings makes gender visible where it otherwise might remain concealed.[95] In the case of

[93] Lidtke, "Burghers, Workers," 29.
[94] Scott, "Women in *The Making of the English Working Class*," 85, 88.
[95] Scott's deconstructive reading of E. P. Thompson's *Making of the English Working Class* offers one excellent example of how this might be done.

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the German Textile Union, gender, embodied in the politics of *Eigenart*, did not represent an obstacle or a barrier to class formation as much as a contestation of the boundaries established by male leaders. If class formation is viewed as a series of makings and remakings, as a process that was constantly contested, either explicitly by those who opposed it or implicitly by those who stood outside it and who appeared as the "other" or the "backward," then gender appears to constitute a continual point of contest, a renewed disordering of the process of class formation.[96] The dichotomies of class and gender, of class and "non-class" distinctions, which are integral to most historical accounts of class formation, obscure the ways in which women appropriated and transformed the vocabulary and ideology of class by inflecting its universal claims, exposing its masculine character, and inscribing class with their own meanings. The alternative to this dichotomous view is one of class formation as a series of short-lived resolutions, new destabilizations, and redefinitions in which gender both shapes and contests class.

[96] Mary Nolan's suggestion that the working class must re-create itself at each stage of industrial capitalism, as the economy and labor force are restructured, as political institutions and forms of hegemony change, prompted me to consider the place of gender in this process. Mary Nolan, *Social Democracy and Society: Working-Class Radicalism in Düsseldorf, 1890–1920* (Cambridge, 1981), 2. On this point, see Sewell's critique of Thompson, "How Classes Are Made," 68.

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Challenging Sexual Discrimination in the Historical Profession: Women Historians and the American Historical Association, 1890–1940

JACQUELINE GOGGIN

FRIENDS AND ADVERSARIES OF WOMEN'S EFFORTS to work as professional historians have generally agreed on the bare facts of women's fate in colleges and universities and in professional associations. The explanations have been another matter. In 1943, William B. Hesseltine and Louis B. Kaplan, of the University of Wisconsin, in their statistical study, "Women Doctors of Philosophy in History," concluded that the average woman scholar "seldom held high or responsible positions in the teaching profession, and she has not proved a productive scholar . . . the teaching of history and historical research are activities in which men engage in greater numbers than women. Women . . . hold poor positions, are more likely to be unemployed, and are less likely to do research than men. Moreover, the historical profession awards less recognition to its women members."[1] Undoubtedly, many of their assertions remain true today, especially those regarding the employment of women historians. Hesseltine and Kaplan did not

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[1] William B. Hesseltine and Louis Kaplan, "Women Doctors of Philosophy in History: A Series of Comparisons," *Journal of Higher Education*, 14 (1943): 257. This article was one of a trilogy on history Ph.D.s by Hesseltine and Kaplan. Also see "Doctors of Philosophy in History: A Statistical Study," *AHR*, 47 (July 1942): 765–800; and "Negro Doctors of Philosophy in History," *Negro History Bulletin*, 6 (December 1942): 59, 67. It should be noted that only one black woman received the Ph.D. in history before 1940 and that this article focuses on white women. Women historians are defined as those holding Ph.D.s in history unless otherwise indicated. Lucy M. Salmon is one of the exceptions discussed, although she held an M.A. and had taken coursework toward the Ph.D.

An assessment of sexual discrimination in the historical profession and its relation to the ideal of objectivity was omitted from the recent study by Peter Novick. Although Novick carefully illuminated the pervasive racism and anti-Semitism among many members of the historical profession in its early years, he ignored prejudice against women, and there always have been more women in the profession than blacks or Jews. See Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (Cambridge, 1988), 172–73, 365–67, 470–510. Novick does pay attention to women's reemergence in the profession in the 1960s and 1970s and to women's history in this period.

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attempt, however, to identify the causes of women's unequal standing in the historical profession. While acknowledging that women had to "persevere" to obtain doctoral degrees, Hesseltine and Kaplan did not suggest that discrimination against women accounted for their lagging performance when compared with male colleagues.

Ironically, the year that Hesseltine and Kaplan's study appeared was the same year that Nellie Neilson, a medievalist from Mount Holyoke College, served as the first female president of the American Historical Association, an attainment that was not to be repeated until the election of Natalie Zemon Davis in 1985. Perhaps more telling is the fact that Neilson's candidacy had been proposed as early as 1932, and it took almost ten years of lobbying by women historians and a few liberal men to elect her. The paucity of women in leadership positions in the AHA, together with the long struggle to achieve even a few appointments, suggests that Hesseltine and Kaplan failed to recognize sexual discrimination when they assessed women's place in the profession.

Almost from its inception, the historical profession has been dominated by white male practitioners. There is little evidence that, prior to 1940, white male historians collectively offered encouragement or support to female colleagues. In fact, the records of the American Historical Association and history departments, and the personal papers of male and female historians reveal that discrimination against women deterred them from realizing fully their scholarly and professional potential as historians.

Women confronted many difficulties as they attempted to gain scholarly credibility and respect from male historians. The intellectual and social isolation imposed by sexual discrimination was a powerful deterrent to sustained scholarly and professional activity. Viewed as intellectually inferior, women had difficulty finding employment commensurate with their education and training. They had difficulty obtaining positions on historical association committees and governing boards, publishing articles in the major historical journals, and presenting papers at meetings of the major historical organizations. When they did attend professional meetings, they were frequently confronted with segregated social functions, such as smokers for men and teas for women. Yet women historians published, served on committees, were elected to office, and presented papers at historical meetings. They also actively protested against sexual discrimination and formed support groups and networks to promote scholarly and professional advancement. They formed alliances with and received support from women outside the historical profession and were active members of women's organizations such as the American Association of University Women, the General Federation of Women's Clubs, the League of Women Voters, the National Woman's Party, and the Women's International League for Peace and Freedom. This essay examines the forms of sexual discrimination in the historical profession from the 1890s to 1940 and discusses the efforts of women to counteract it, focusing on three areas: the education and employment of women, their research and publications, and their professional involvement in the AHA.[2]

[2] The impact of professionalization on women's intellectual and personal opportunity recently has received attention, notably from Margaret Rossiter and Rosalind Rosenberg. See Rossiter, *Women*

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THE ENTRANCE OF WOMEN INTO THE HISTORICAL PROFESSION was part of the expansion of higher education in the mid-nineteenth century and increasing acceptance of the appropriateness of such education for women. Because only two women's colleges, Bryn Mawr and Radcliffe, granted the Ph.D. in history, most women obtained their degrees from coeducational institutions, especially those in the Midwest. Kate Ernest Levi became the first woman to receive the Ph.D. in history in 1893 at the University of Wisconsin. By 1900, 8 women had received the degree. An additional 24 women were trained by 1910, and by 1920, their number had increased by 49. During the period under consideration, the largest enrollment in graduate degree programs in history for both sexes came after World War I. From 1920 to 1930, 131 new women Ph.D.s in history were trained, and from 1931 to 1935, an additional 122 women earned doctorates. From 1920 to 1929, almost 22 percent of the doctorates in history were awarded to women. Thus, from 1893 until 1935, 334 women received the Ph.D. in history compared with 1,721 men. Put another way, women received 16 percent of the Ph.D.s awarded during this period.[3]

A detailed statistical study outlining the educational backgrounds, ages, marital status, employment patterns, and career histories of these 334 women, though badly needed, is beyond the scope of this essay. Nevertheless, some broad generalizations about their education and careers can be made using Hesseltine and Kaplan's data and a variety of archival sources. It appears that while the majority of women historians were trained at coeducational institutions, few found employment at these institutions. Those that did tended to teach in the midwestern colleges and universities established in the mid-nineteenth and early twentieth centuries. Most female historians were employed in high schools, libraries and archives, and women's colleges, where they made signal contributions to the development of the field. Hesseltine and Kaplan found that as of 1935, no women historians were employed in men's colleges.[4]

Scientists in America: Struggles and Strategies to 1940 (Baltimore, Md., 1982); Rosenberg, *Beyond Separate Spheres: Intellectual Roots of Modern Feminism* (New Haven, Conn., 1982). For a good summary of major works, see Joan Jacobs Brumberg and Nancy Tomes, "Women in the Professions: A Research Agenda for American Historians," *Reviews in American History*, 10 (June 1982): 275–96. Also see Virginia Drachman, *Hospitals with a Heart: Women Doctors and the Paradox of Separatism at the New England Hospital, 1862–1969* (Ithaca, N.Y., 1984); Regina Markell Morantz-Sanchez, *Sympathy and Science: Women Physicians in American Medicine* (New York, 1985); Ellen Fitzpatrick, *Endless Crusade: Women Social Scientists and Progressive Reform* (New York, 1990); Mary Jo Deegan, *Jane Addams and the Men of the Chicago School, 1892–1918* (New Brunswick, N.J., 1988).

[3] Hesseltine and Kaplan, "Women Doctors of Philosophy," 254–59. Among the most useful and brief treatments on higher education for women are Eleanor Flexnor, *Century of Struggle* (New York, 1973); Helen Astin, *The Woman Doctorate in America* (New York, 1969); Barbara Miller Solomon, *In the Company of Educated Women* (New Haven, Conn., 1985); Joyce Antler, "Culture, Service, and Work: Changing Ideals of Higher Education for Women," in Pamela J. Perun, ed., *The Undergraduate Woman: Issues in Educational Equity* (Lexington, Mass., 1982): 15–44; Patricia Albjerg Graham, "Expansion and Exclusion: A History of Women in American Higher Education," *Signs*, 3 (Summer 1978): 759–73; Jill Kerr Conway, "Pespectives on the History of Women's Education in the U.S.," *History of Education Quarterly*, 14 (1974): 1–12; Mary J. Oates and Susan Williamson, "Women's Colleges and Women Achievers," *Signs*, 3 (Summer 1978): 795–806; Roberta Wein, "Women's Colleges and Domesticity," *History of Education Quarterly*, 14 (Spring 1974): 31–47. Also see Patricia Albjerg Graham, "Revisiting the Rose Report," *Coordinating Committee on Women in the Historical Profession Newsletter*, 18 (February 1987): 7–10.

[4] Hesseltine and Kaplan, "Women Doctors of Philosophy," 254–59. Among the archival sources

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Although leading male historians at the most prestigious colleges and universities in the country trained the first generations of female historians and helped female students in their search for fellowships and employment, often praising their scholarship, these men did little collectively to reduce the discrimination women faced in their quest to be treated as equals. Frederick Jackson Turner, Carl Russell Fish, and William E. Dodd trained many women, and others, such as Arthur M. Schlesinger, Sr., and Merle Curti, promoted the study of women's history. Before 1940, however, most male historians offered little encouragement or support to female colleagues but instead fostered and contributed in a variety of ways to the continuous sexual discrimination against women historians.[5] In letters of recommendation for fellowships and jobs, male historians often commented on the age, appearance, dress, personality, and character of the female applicant in a manner that would have been considered improper when writing on behalf of men. Conversely, they paid less attention to teaching ability and research skills. The letter that Professor Julian P. Bretz of Cornell University wrote on behalf of his student Caroline Lewis was typical: "Miss Lewis is about thirty years of age. She is somewhat more pleasing in manners and appearance than the average student of her kind, and is, on the whole, teachable and easy to get along with." Even when they intended to be complimentary, many men wrote letters that were less than flattering. Ephraim D. Adams of Stanford University indicated that Latin Americanist Mary Wilhelmine Williams was "a very capable, and I think, unusual woman, not especially attractive in appearance, yet not unattractive in any special sense either."[6] Similarly, James A. Robertson, editor of the *Hispanic American Historical Review*, when writing to the Guggenheim Foun-

consulted were the Records of the American Historical Association, Manuscript Division, Library of Congress (hereafter cited, AHA Records); the John Franklin Jameson Papers, Manuscript Division, Library of Congress (hereafter cited, JFJ Papers); the James Alexander Robertson Papers, Manuscript Division, Library of Congress (hereafter cited, Robertson Papers); the William E. Dodd Papers, Manuscript Division, Library of Congress (hereafter cited, Dodd Papers); Louise Phelps Kellogg Papers, State Historical Society of Wisconsin, Madison (hereafter cited, Kellogg Papers); the Carl Russell Fish Papers, State Historical Society of Wisconsin, Madison (hereafter cited, Fish Papers); the Merle Curti Papers, State Historical Society of Wisconsin, Madison (hereafter cited, Curti Papers); August C. Krey Papers, Walter Library, University of Minnesota (hereafter cited, Krey Papers); and the records of the Berkshire Conference of Women Historians, Schlesinger Library, Radcliffe College, Cambridge, Mass. (hereafter cited, Berkshire Conference Records).

[5] I am currently compiling a profile of the men who were supportive mentors of women. Tentatively, I can say that many had wives who were well-educated professionals and activists, like Arthur M. Schlesinger, Sr., and Merle Curti, or they had class origins like William E. Dodd, who was the son of North Carolina yeomen farmers, and thus may have had more sympathy for the disadvantaged. See "Arthur M. Schlesinger, Jr. Reassesses His Father's Work," *Radcliffe News* (Spring 1989): 5; Arthur M. Schlesinger, Sr., *The Rise of the City, 1878–1898* (New York, 1933), Chapter 5, "The American Woman," 121–59; Robert Dallek, *Democrat and Diplomat: The Life of William E. Dodd* (New York, 1968); Jacqueline Goggin, interview with Merle Curti, June 21, 1985, Madison, Wisconsin. Mary Jo Deegan also argues that men who supported women sociologists at Chicago had wives or daughters who were professionals and activists in the suffrage movement and had written on issues of concern to women. See Deegan, *Jane Addams and the Men of the Chicago School*, 179–208. David D. Van Tassell told me that as late as the 1960s at the University of Texas, women were discouraged from graduate study. Generally, male faculty believed women would get married and never finish their degrees; he and his colleagues genuinely believed they had women's best interests in mind. Goggin, conversation with Van Tassell, December 1, 1988, Washington, D.C.

[6] Julian P. Bretz to J. Franklin Jameson, February 25, 1913, JFJ Papers; Ephraim D. Adams to Annie Heloise Abel, December 26, 1913, Department of History Archives, Stanford University.

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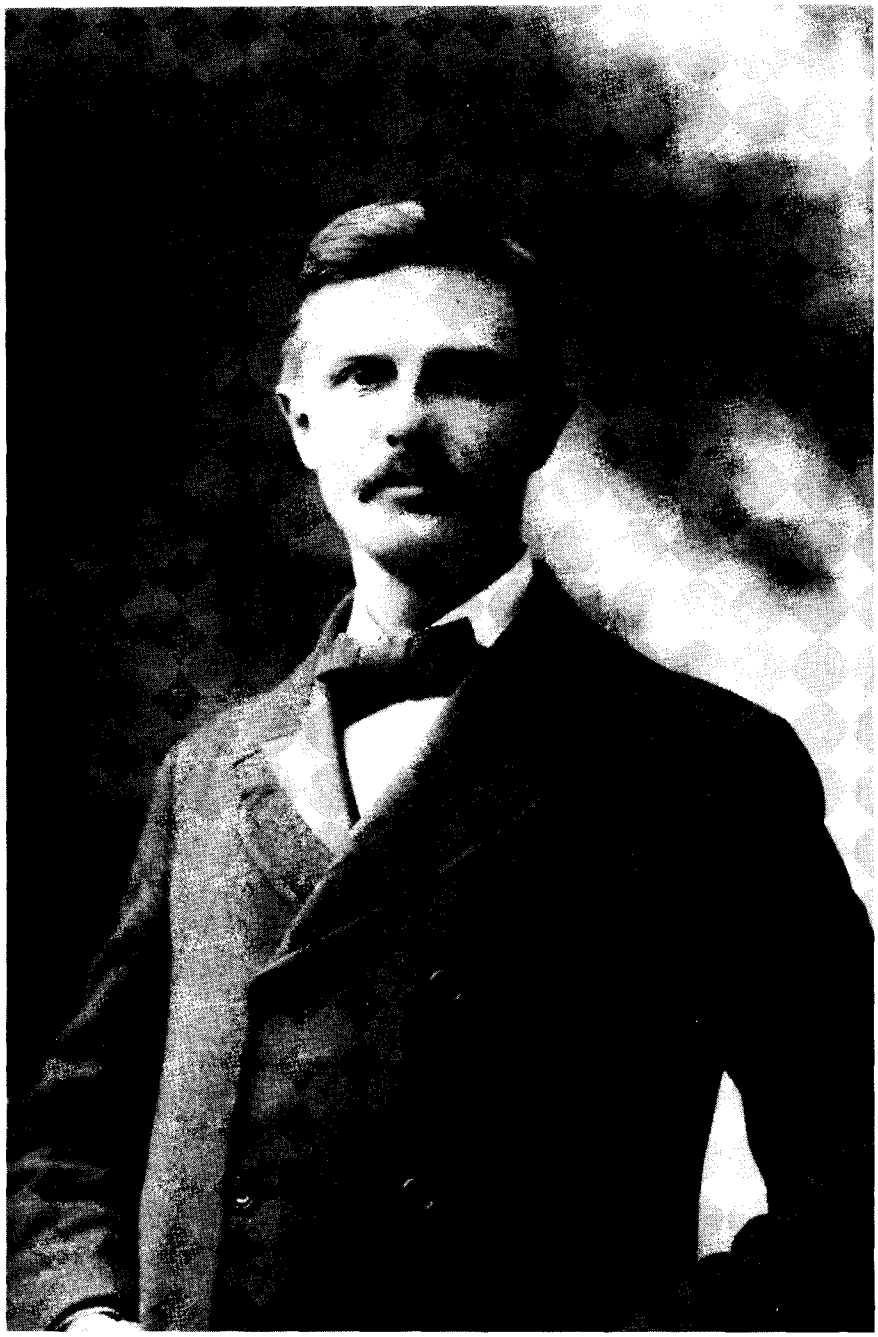


FIGURE 1: Frederick Jackson Turner, about 1900–1905. Courtesy of the State Historical Society of Wisconsin, WHi(X3)40154.

dation about Kathryn Abby, asserted that she would "go as far as any woman historical teacher in the U.S." and that she was "free from any sex suggestions—basing her judgments solely on their merits."[7] When searching for new faculty,

[7] James A. Robertson to Henry Allen Moe, December 9, 1930, Robertson Papers. The same types

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FIGURE 2: Merle Curti, about 1950, University of Wisconsin professor. Courtesy of the State Historical Society of Wisconsin, WHi(X3)30188.

of letters were written about blacks and Jews. Harvard University professor Edward Channing, who trained the African-American historian from Virginia Carter G. Woodson, wrote a letter of recommendation contending that, as a "light-colored mulatto," Woodson obviously was "related to some of the best families of Virginia." See Edward Channing to Max Farrand, October 14, 1920, Channing Papers, Harvard University Archives, Pusey Library, Harvard University. Similarly, Peter Novick found many letters of recommendation about Jewish scholars asserting that the individual in question did not act or look Jewish; Novick, *That Noble Dream*, 172–73.

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male historians unabashedly expressed a preference for men, generally believing that women were inferior and difficult to get along with. Despite the beliefs of a few men that some women might be their intellectual equals, most advised their women students to be satisfied with a second-class status of limited employment prospects. In their interactions with and private correspondence about women scholars, male historians expressed regret that the most prestigious colleges and universities preferred to hire men, but the intensity of their regret did not extend to making an effort to change the prevailing climate of opinion. In fact, the reluctance of supportive men to speak out against discrimination toward women reinforced the practice.

After Mary P. Clarke went on to study for the Ph.D. under Charles McLean Andrews at Bryn Mawr in 1907, Wilbur C. Abbott, her M.A. adviser at the University of Kansas, thought "[i]t would . . . be a distinct loss for her to go into high school teaching after such training."[8] Similarly, H. L. Gray of Bryn Mawr urged J. Franklin Jameson to consider his student Dorothy Vastine for a position in the Library of Congress Manuscript Division even though "her ability should fit her for teaching."[9] When Lawrence M. Larsen of the University of Illinois wrote to August C. Krey of the University of Minnesota in 1928 to inquire about "men" for a position at Illinois, Krey replied that he was "almost tempted to say that most of the best men who have taken their doctorate at Minnesota during the last five years have been women."[10] Krey later tried to obtain a position for one of his female students at the College of William and Mary but discovered that "[a]pparently they are not yet sufficiently emancipated to consider a woman for their position."[11] Even Caroline von Ranke, granddaughter of Leopold von Ranke, who was seeking employment as a historian in the United States in the 1920s, received no special consideration. She was advised to confine her search to women's colleges.[12]

Once a woman had spent a considerable part of her career at a women's college, it became difficult, if not impossible, to obtain a position elsewhere. Women's colleges both affirmed and denied women's status in the profession. As other historians have demonstrated, women's colleges were "Adamless Edens," places of "Splendid Isolation," where intellectual women supported each other and fostered sustained scholarly activity. At the same time, however, these institutions provided daily confirmation of women's limited opportunities in the profession. After teaching at Goucher College (formerly the Woman's College of Baltimore) for more than twelve years, for example, Mary Wilhelmine Williams unsuccess-

[8] Wilbur C. Abbott to J. Franklin Jameson, August 10, 1907, AHA Records. Ellen Fitzpatrick also has argued that the sociologists at the University of Chicago trained several outstanding women and recognized their intellectual abilities yet were not anxious to employ them at the University of Chicago and did not challenge the prevailing social conventions regarding women's employment; Fitzpatrick, *Endless Crusade*, 72–73.

[9] H. L. Gray to J. Franklin Jameson, September 17, 1932, Manuscript Division Records, Administrative Files, Library of Congress Archives (hereafter cited as LC Archives).

[10] August C. Krey to Lawrence M. Larsen, March 14, 1928, Krey Papers.

[11] Krey to Gertrude Doxey, April 29, 1935, Krey Papers.

[12] On behalf of Ranke, J. Franklin Jameson had written several colleagues to inquire about job prospects. William Boyd, of Trinity College in North Carolina, replied: "We must have a man as most of the work is with men. However, I will keep her case in mind. Very often we are requested to name someone to the Southern women's colleges." Boyd to Jameson, May 22, 1923, JFJ Papers.

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fully tried to obtain a position at a coeducational institution. She was over fifty years old when she began her search, and age was another mark against her.[13]

Despite support from male advisers, women often became discouraged about job prospects. Martha L. Edwards, a graduate student at the University of Wisconsin and a co-donor with Florence Porter Robinson of the chair in women's history there, wrote to her adviser Carl Russell Fish in 1915 that while she was most qualified to teach, she believed the "misfortune of being a woman would make it exceedingly difficult for [her] to find a teaching position."[14] After she finished her degree, Edwards hoped to obtain a position at Sophie Newcomb College in New Orleans, but she was informed that the administrators preferred to hire men.[15]

During the depression, the job market for women shrank further when small institutions were forced to cut back on hiring, and, for the first time, women's colleges regularly began to hire men over women. Before the 1930s, the majority of female historians remained single. The few who were married often kept their marriages a secret, for it was assumed that they did not need to work and would be taking jobs from married men who presumably had families to support. Eleanor B. Scott of Florida State College for Women lost her job in 1933 and complained, "Even in a woman's college . . . , women do not get the breaks."[16] When men in women's colleges recommended female historians for jobs during the depression, administrators felt that greater prestige would be brought to the institution if a male were hired instead. Merle Curti left Smith College to go to

[13] See Penina Migdall Glazer and Miriam Slater, *Unequal Colleagues: The Entrance of Women into the Professions, 1880–1940* (New Brunswick, N.J., 1987), 25–68; Patricia Palmieri, "Here Was Fellowship: A Social Portrait of Academic Women at Wellesley College, 1895–1920," *History of Education Quarterly*, 23 (Summer 1983): 195–214; and Palmieri, "In Adamless Eden: A Social Portait of the Academic Community at Wellesley College, 1875–1920" (Ed.D. thesis, Harvard University, 1981); Mary W. Williams to James A. Robertson, November 20, 1927, Robertson Papers. A few years earlier, Ephraim Adams had nominated Williams for the newly endowed Alice Freeman Palmer chair at the University of Michigan, although the income generated from the endowment was insufficient to fill the chair. See Adams to Claude H. Van Tyne, January 24, 1925, Van Tyne Papers, Michigan Historical Collections, Bentley Historical Library, University of Michigan. Age also was a factor in preventing Nannie M. Tilley from obtaining a position at Goucher College in 1940. She worked for many years as a curator of manuscripts at Duke University. See Richard Shryock to Nannie M. Tilley, May 2, 1939, Shryock to Ella Lonn, January 16, 1940, Shryock to Katherine J. Gallagher, January 16, 1940, Dorothy M. Quynn to Shryock, February 21, 1940, and Shryock to Quynn, February 28, 1940, all in Shryock Papers, American Philosophical Society Library, Philadelphia, Pennsylvania.

[14] Martha L. Edwards to Carl Russell Fish, January 27, 1915, Fish Papers. Edwards was also hearing impaired. She eventually received a job at Lake Erie Junior College in Ohio.

[15] To his credit, Fish wrote to M. J. White of Sophie Newcomb College to inquire about job prospects on Edwards' behalf. White replied: "I am sorry to report that . . . all things being equal, the authorities prefer male to female professors." See M. J. White to Carl Russell Fish, February 15, 1915, Fish Papers.

[16] Eleanor B. Scott to James A. Robertson, March 9, 1933, Robertson Papers. Of 229 women Ph.D.s in all fields in 1900, only 16 were married; in 1924, of 1,600 Ph.D.s, only 400 were married. See Solomon, *In the Company of Educated Women*, 135–39. On women's employment during the depression, see, for example, Winifred Wandersee, *Women's Work and Family Values, 1920–1940* (Cambridge, Mass., 1981); and Morantz-Sanchez, *Sympathy and Science*, 328–32, where she also argues that female physicians' personal lives were increasingly difficult by the 1930s. In a letter I received on July 28, 1987, Elizabeth Fox-Genovese told me that Caroline Robbins, the distinguished European historian, kept her marriage a secret for years. Also see Fox-Genovese, *Feminism without Illusions: A Critique of Individualism* (Chapel Hill, N.C., 1991), 246–47. Even in the 1960s, men continued to assume that women were taking jobs and places in graduate school that should have been reserved for them.

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Columbia University in 1936 and recommended that Kathleen Bruce of Hollins College, a pioneer in the industrial history of the Old South, be hired to replace him. He was replaced with another man. Disappointed, Bruce remarked that "[i]t is paradoxical and rather ironical, as [women's colleges] are trainers of women for the very posts which they are now closing to them."[17] Blanche Henry Clark was teaching at an exclusive girls' secondary school in Nashville in the late 1930s and felt fortunate just to have a job. She realized "how hard it is for a girl to be placed."[18] James Alexander Robertson confided to one woman historian who lost her job in the 1930s that "the depression is being made to cover a multitude of sins, in some cases," he thought, "unnecessarily."[19]

Because jobs were scarce during the depression, even positions outside of the academy were regarded as suitable only for men. When Julian P. Bretz recommended his student Margaret Plunkett for the position of assistant director of Historic Sites and Buildings in the National Park Service, Waldo G. Leland of the search committee refused to consider her. Leland believed that a woman could not successfully undertake the job, because "[i]t requires endless travel, putting up with all kinds of conditions, roughing it on occasion, and accepting accommodations with Superintendents of Parks and Monuments under circumstances that would be almost impossible for a woman."[20]

Women also found it more difficult to obtain fellowships during the depression. As job opportunities were increasingly limited, universities and foundations were less willing to invest in women. Alice Kimball Smith, a graduate student at Yale, was denied a fellowship because she was married. Kimball recalled that she "raised [her] eyebrows but not [her] voice" and was given $100 for books because she was the best first-year student. Elizabeth Hoon, Ph.D. student of Charles McLean Andrews, also at Yale, was not hopeful of obtaining a research fellowship to complete her dissertation, for she had been "advised that the fellowship has been awarded to but one woman, and usually to an older man."[21] Similarly, Dorothy Dodd, an undergraduate student of Kathryn Abby at Florida State College for Women, applied for a graduate fellowship at the University of North Carolina but was rejected because they did not award fellowships to women. She went on to seek the Ph.D. at the University of Chicago, where she studied under William E. Dodd, who nurtured her ambitions and supported her.[22]

When women succeeded in finding academic employment, and only half of all those with the Ph.D. had done so by 1939 (over one-third were unemployed), they

[17] Kathleen Bruce to Merle Curti, April 29, 1936, Curti Papers.

[18] Blanche Henry Clark to Richard Shryock, April 5, 1939, Shryock to Clark, June 12, 1939, and Clark to Shryock, June 28, 1939, all in Shryock Papers.

[19] James A. Robertson to Eleanor B. Scott, March 30, 1933, Robertson Papers.

[20] Waldo G. Leland to J. Franklin Jameson, May 1, 1936, Julian P. Bretz to Jameson, April 27, 1936, both in JFJ Papers.

[21] Alice Kimball Smith, "Limited Opportunities," review of Margaret Rossiter, *Women Scientists in America, New York Times Book Review* (February 6, 1983): 12, 21 (first quotation); Elizabeth Hoon to Charles McLean Andrews, February 19, 1932 (second quotation), Hoon to Andrews, August 14, 1930, and March 21, 1932, all in Charles McLean Andrews Papers, Sterling Memorial Library, Yale University.

[22] Dorothy Dodd to James A. Robertson, November 13, 1929, and Kathryn Abby to Robertson, December 24, 1929, both in Robertson Papers. Female medical students also had a more difficult time obtaining research support during this era. See Morantz-Sanchez, *Sympathy and Science*, 330.

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were frequently confronted with heavy teaching loads, committee work, and poor library facilities that limited their ability to do research and writing. Annie Heloise Abel confided to her Yale Ph.D. adviser, Edward G. Bourne, that she made trips to Ithaca twice a month to use the library at Cornell University because of inadequate library facilities at Wells College, the women's college in Aurora, New York, where she taught. Her situation improved when she moved to Goucher College, close to the Library of Congress and federal government records in Washington, D.C.[23] Amelia C. Ford, who received her Ph.D. at Wisconsin in 1908 under Frederick Jackson Turner, wrote to J. Franklin Jameson in 1928 to ask about research fellowships. Ford had been teaching at Milwaukee Downer College for Women for twenty years and claimed that her teaching duties absorbed all her time and energy. She confessed to being "a member of that undesirable class of unproductive doctors of philosophy."[24] When James A. Robertson invited Lillian E. Fisher of Oklahoma College for Women to contribute an article to the *Hispanic American Historical Review*, she declined because she did "not have a chance to do any writing in the winter."[25] Many male historians faced similar problems, but virtually all women did.

The women who obtained employment in coeducational institutions faced additional obstacles. Usually the only women in their departments, they were treated as inferiors and were subject to overt forms of discrimination. Dorothy Louise Mackay Quynn, who was employed at West Virginia University before she moved on to Duke University, believed she had to write a letter of apology to explain why she failed to show up at a luncheon given for fellow medievalist August C. Krey when he spoke at Duke in 1940: "I am almost never included in department gatherings of a professional nature, and am, I know, too sensitive on the subject . . . I usually pretend not to hear remarks such as that made about 'the luncheon given for Mr. Krey by the Department on Thursday.'"[26]

WOMEN WHO TAUGHT IN WOMEN'S COLLEGES and trained successive generations of historians as undergraduates helped soften the blows their students would face in graduate school. Both Mary Williams and Viola Barnes taught women's history. Williams believed that women's history must become known before society would change, and she advocated its teaching long before it gained acceptance by the leaders of the historical profession. She taught "The History of the Woman Movement" at Goucher College for twenty years.[27] Williams and others remained

[23] Annie Heloise Abel to Edward G. Bourne, March 15, 1906, April 16, 1906, May 8, 1906, all in Edward G. Bourne Papers, Sterling Memorial Library, Yale University; Abel to Charles H. Haskins, January 28, 1907, AHA Records.

[24] Amelia C. Ford to J. Franklin Jameson, January 29, 1928, JFJ Papers.

[25] Lillian E. Fisher to James A. Robertson, November 10, 1931, Robertson Papers.

[26] Dorothy Louise Mackay Quynn to August C. Krey, February 22, 1940, Krey Papers. In a report to the AHA, Joan Jenson, chair of the Committee on Women Historians, writing about her experiences in the 1950s, expressed sentiments similar to Quynn's: "[L]ike other women graduates, I expected little from our profession. We hid our injuries, complained quietly over coffee, and found our strength elsewhere." See "Committee on Women Historians, 1970–1990: A Twenty Year Report," *Perspectives* (March 1991): 8.

[27] Mary W. Williams to Allen Johnson, November 6, 1925, and the course description for "History

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FIGURE 3: Viola Barnes and students in Civil War history class circa 1949. Courtesy of Mount Holyoke College Archives, South Hadley, Massachusetts.

optimistic that men's attitudes toward them were changing, albeit at a snail's pace, and they were instrumental in encouraging students to seek the Ph.D. Williams sent several Goucher undergraduates on to graduate study, as did Nellie Neilson, whose special gift and love for medieval history inspired some women to specialize in that subject at the graduate level. Lucy Salmon sent many of her Vassar College students to the University of Wisconsin, not only to work with Carl Russell Fish, whom she admired, but because Louise Phelps Kellogg, a student of Frederick Jackson Turner at Wisconsin who remained in Madison to work at the State Historical Society, agreed to serve as a mentor for Salmon's students.[28] Others

82," copy from Goucher catalog, records of the American Council of Learned Societies, Manuscript Division, Library of Congress (hereafter cited, ACLS Records); and copy of course syllabus, "History 82, The History of the Woman Movement in the United States," *ca.* 1933, Mary W. Williams Papers, Stanford University Archives. This copy has been dated according to the date of the most recent books listed on the reading list. Unfortunately, earlier and later versions of the syllabus do not survive for comparison or analysis of any changes that Williams might have made. Also see Viola Barnes to Carrie Chapman Catt, December 23, 1940, Carrie Chapman Catt Papers, Manuscript Division, Library of Congress (hereafter, Catt Papers).

[28] Lucy Salmon to Louise Phelps Kellogg, August 18, 1907, September 16, 1910, May 30, 1917, all in Kellogg Papers; Martha L. Edwards to Carl Russell Fish, August 10, 1914, Salmon to Fish, November 27, 1917, both in Fish Papers; Mary Wilhelmine Williams to Waldo G. Leland, May 4, 1918, AHA Records; William Roy Smith to J. Franklin Jameson, September 12, 1933, Dorothy Vastine to Jameson, September 8, 1933, both in LC Archives. William Roy Smith wrote that Vastine

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served as mentors for younger women whom they had not trained as undergraduates. After receiving some measure of scholarly respect, they took the initiative in promoting the work of younger colleagues. Shirley Farr of the University of Chicago, for example, established the Farr Fellowship for Women Historians in 1924; it provided living expenses for a female historian to spend a year conducting research in Washington, D.C.[29]

Throughout their careers, both Lucy Salmon and Mary Williams recommended women for prizes and fellowships and suggested that women be asked to give papers at meetings of the AHA and contribute to the *American Historical Review*, the *Hispanic American Historical Review*, and the *Dictionary of American Biography*. Often, their recommendations fell on deaf ears, for promotion by and recommendation of a male adviser or mentor was taken more seriously.[30]

Male historians served as the gatekeepers for the profession, not only by defining history as a male enterprise but also by writing history that dealt mainly with men and male institutions, while purporting to deal with all of society. They determined what constituted history in all its various subfields and specializations. As editors and reviewers for the *AHR* and the *Mississippi Valley Historical Review*, they decided what would be published and fostered editorial policies that excluded women historians and women's history. Unlike the contemporary practice of scholarly journals, where articles are sent out anonymously for review, in the early years reviewers knew whose work they were evaluating. If scholarly productivity is measured in terms of books and articles published in the two major journals, women lagged far behind their male colleagues.[31] Of the 695 articles

was "keenly interested in American History and I dare say that she might have majored in that subject if she had not, at an early period in her career, fallen under the magnetic influence of Dr. Nellie Neilson of Mount Holyoke." Neilson "not only has an uncanny faculty for inspiring her students with a love for mediaeval history, but she also teaches them how to use manuscript material and enjoy the game." Also see Glazer and Slater, *Unequal Colleagues*, 25–68, on Neilson.

[29] Farr wished to remain anonymous and asked the well-connected J. Franklin Jameson to select the recipient and administer the fellowship. During the early years, Jameson solicited advice on candidates from male colleagues. Beginning in 1928, he sought advice from a few women—among them were Elizabeth Donnan of Wellesley and Eloise Ellery of Mount Holyoke. An entire folder of correspondence on the Farr Fellowship can be found in the JFJ Papers. At her death in 1955, Farr bequeathed $300,000 to the American Association of University Women for women's education. See her obituary in the *Journal of the American Association of University Women*, 50 (October 1956): 65.

[30] Charles McLean Andrews to Jameson, June 3, 1895, July 31, 1895, November 23, 1895, October 16, 1896, November 30, 1896, January 20, 1897, all in JFJ Papers; Lucy Salmon to Andrew C. McLaughlin, November 22, 1902, February 9, 1905, both in AHA Records; Salmon suggested that McLaughlin ask Frances G. Davenport, Eloise Ellery, and Susan M. Kingsbury to write book reviews: "I am interested in all of these women who are not only 'promising' but have fulfilled these promises in a measure." See Salmon to Jameson, January 20, 1906, Jameson to Annie Heloise Abel, February 2, 1906, both in AHA Records. Abel had won the Justin Winsor Prize in 1906 for her Ph.D. thesis, "The History of the Events Resulting in Indian Consolidation West of the Mississippi River." See Charles H. Hull to Edward G. Bourne, December 18, 1906, Bourne Papers; Salmon to Jameson, July 8, 1907, Jameson to Charles McLean Andrews, August 26, 1911, Waldo Leland to Jameson, July 10, 1917, all in AHA Records; Mary W. Williams to James A. Robertson, December 30, 1919, Robertson to Williams, January 3, 1920, both in Robertson Papers; Ella Lonn to Allen Johnson, December 8, 1927, ACLS Records.

[31] See Hesseltine and Kaplan, "Women Doctors of Philosophy," 254–59; Helen L. Manning to Jameson, May 1, 1896, JFJ Papers; Louise Ropes Loomis to Jameson, June 20, 1907, Louise Phelps Kellogg to Jameson, October 29, 1908, Jameson to Andrew C. McLaughlin, August 31, 1909, Nellie Neilson to Jameson, February 1, 1919, Jameson to Neilson, February 25, 1919, Jameson to Carl

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published in the *AHR* from 1895 until 1940, only 21, or 3 percent, were by women, and women comprised 15 to 20 percent of the members of the organization during this period. Similarly, from 1914 to 1940, women published only 19, or 3.7 percent, of the 510 articles in the *MVHR*. Women did publish more frequently in journals like the *HAHR* and the *Journal of Negro History*, and examination of other specialized journals would probably reveal a larger percentage of articles by women.[32]

Not surprisingly, the *AHR* and the *MVHR* also paid little attention to women's history. Prior to 1940, only three articles about women appeared in the two journals, none written by a woman historian. But women historians did not ignore women's history. While their scholarship did not find its way into the *AHR* or the *MVHR*, it was incorporated into monographs, textbooks, and sketches written for biographical and historical dictionaries. Mary Williams, for example, wrote sketches of "those who have played a major part in the history of the woman movement" for the *Dictionary of American Biography* and "most of the feminist articles," as she termed them, for the *Dictionary of American History*, including those on the "Nineteenth Amendment" and "Married Women's Property Rights." By the 1930s, a handful of dissertations and books on women had been written by female historians. Their early efforts to write the history of their sex were largely ignored by the historical profession and only recently has their work received much attention.[33]

Becker, June 11, 1921, Ella Lonn to Jameson, February 25, 1922, Jameson to Lonn, March 11, 1922, Martha L. Edwards to Jameson, December 13, 1923, Hannah G. Roach to Jameson, September 11, 1924, Kathleen Bruce to Jameson, January 14, 1926; Jameson to Bruce, February 17, 1926, all in AHA Records; Bruce's article on slave labor in the Virginia iron industry was published in the *William and Mary Quarterly*; Laura A. White to Jameson, October 28, 1927, Jameson to White, November 9, 1927, both in AHA Records. Also see Morey D. Rothberg, "'To Set a Standard of Workmanship and Compel Men to Conform to It': John Franklin Jameson as Editor of the *American Historical Review*," *AHR*, 89 (October 1984): 957–75.

[32] While compiling similar information on black historians' publications in the *MVHR* and the *AHR*, I also examined the publication records of women historians. See Jacqueline Goggin, "Carter G. Woodson and the Movement to Promote Black History" (Ph.D. dissertation, University of Rochester, 1984), 93–146; and Goggin, "Countering White Racist Scholarship: Carter G. Woodson and the *Journal of Negro History*," *Journal of Negro History*, 68 (1983): 355–75. During Woodson's editorship of the *JNH*, from 1915 to 1950, 54 of 623 articles, or 9 percent, were by women, although most were not professionally trained historians. The two professionally trained were Annie Heloise Abel-Henderson and Mary W. Williams. On the *HAHR*, see James A. Robertson to Mary Williams, January 3, 1920; Vera Lee Brown to Robertson, April 7, 1923, Robertson to Brown, April 29, 1927, Robertson to Lillian E. Fisher, December 2, 1927, Robertson to Vera Lee Brown, December 7, 1927, all in Robertson Papers.

[33] Lester Shippee, "Jane Grey Swisshelm, Agitator," *MVHR*, 7 (1920–21): 206–27; Bertha-Monica Sterns, "Early Western Magazines for Ladies," *MVHR*, 18 (1931–32): 319–30; and Sterns, "Reform Periodicals and Female Reformers, 1830–1860," *AHR*, 37 (1931–32): 678–99. Sterns was a professor of English at Wellesley College. On women's writings on women's history, see, for example, Lucy Maynard Salmon, *Domestic Service* (New York, 1897); Elizabeth Anthony Dexter, *Colonial Women of Affairs: A Study of Women in Business and the Professions in America before 1776* (Boston, 1924); Julia Cherry Spruill, *Women's Life and Work in the Southern Colonies* (Chapel Hill, N.C., 1938). Also see Bonnie G. Smith, "The Contribution of Women to Modern Historiography in Great Britain, France, and the United States, 1750–1940," *AHR*, 89 (June 1984): 709–32; and Bonnie G. Smith, "History Written and Professed: Women Inside and Outside the Academy, 1880–1930" (Unpublished paper presented at the meeting of the American Historical Association, San Francisco, December 1989, in author's possession). Mary W. Williams wrote biographical sketches of Margaret Mercer, Lucretia Coffin Mott, Ernestine L. Rose, Abby H. Smith, Elizabeth Cady Staton, Eliza Daniel Stewart, Lucy

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Women generally were not encouraged to write about women's past, and many did not think that scholarly study of their sex was an important research priority. Penina Glazer and Miriam Slater, biographers of medievalists Nellie Neilson and Bertha Putnam, for example, argue that while the most talented women recognized that women had a distinctive experience in history, their marginalized position in the profession made them reluctant to challenge prevailing interpretations. Some men also believed that women might not be objective if they wrote women's history. In 1916, for example, when the A. C. Nichols Company asked Jameson to recommend a historian to write on the history of the woman suffrage movement, he recommended Edward Turner of the University of Michigan, who had published a history of the movement in New Jersey. Jameson added that he knew of no one else who was already interested in the topic, except Annie Heloise Abel, then of Smith College, and was not confident "of her ability to take an entirely broad view of the subject."[34]

More surprising is the fact that a few men encouraged women to write dissertations on American women, including Elizabeth Anthony Dexter and Julia Cherry Spruill on colonial women and Virginia Gearheart on antebellum southern women. All three scholars, however, had difficulty obtaining financial support, although Dexter and Spruill eventually published books that have become classics in women's history. Other women, such as Guion Johnson, of the University of North Carolina, wrote social history that included women. As social history was a newly emerging field, it is uncertain whether it was considered particularly suitable for women or if they, and their male advisers, believed the field to be intrinsically worthy.[35]

Although women failed to achieve much prominence within the profession when they wrote on the history of women, many applied their historical skills in the larger society. They wrote histories for and served as advisers to women's organizations and urged them to collect and deposit in libraries their organiza-

Stone, and Eliza Jane Sunderland for the *Dictionary of American Biography*. See Williams to Allen Johnson, November 6, 1925, in ACLS Records; and Williams to Martha Souder, March 16, 1938, Records of the National Woman's Party, Manuscript Division, Library of Congress (hereafter cited as NWP Records).

[34] See Glazer and Slater, *Unequal Colleagues*, 52–55; Jameson to A. C. Nichols Company, June 22, 1916, JFJ Papers; Edward D. Turner, *A History of the Woman's Suffrage Movement in New Jersey* (Northampton, Mass., 1916). The opposite was true of black historians, who were required to write on black topics.

[35] Dexter failed to receive the Justin Winsor Prize, Gearheart was unsuccessful in obtaining a dissertation fellowship, and Spruill did not receive an ACLS fellowship. See Elizabeth Anthony Dexter to the Justin Winsor Prize Committee, March 24, 1924, AHA Records; Virginia Gearheart to J. Franklin Jameson, May 3, 1927, JFJ Papers; and Robert D. W. Connor to Donald Goodchild, January 31, 1936, ACLS Records. Some of the most important work in American social and women's history done by women historians trained in the South was influenced by Schlesinger, Sr. An exchange fellowship provided funds for Harvard graduate students studying southern history to spend a year in residence at the University of North Carolina or Duke University, and students at these universities, in turn, spent a year working with Schlesinger at Harvard. At least eight women held these fellowships beginning in 1928, including Julia Cherry Spruill. At the time that Spruill and others were working under Schlesinger, he was editing with Dixon Ryan Fox the first important series in social history, *A History of American Life* (1927–1948), of which his *Rise of the American City*, with its chapter on women, was a part. Also see Guion G. Johnson's 1978 oral history, in the Southern Oral History Project, Southern Historical Collection, University of North Carolina, Chapel Hill.

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tional records and personal papers. Several women's organizations sponsored publications, plays, and pageants that focused on American women and sought advice from women historians. Mary Williams performed this role for the National Woman's Party and the Women's International League for Peace and Freedom. Similarly, Louise Phelps Kellogg advised the Wisconsin Women's Christian Temperance Union, the Wisconsin Woman's Suffrage Association, and the Wisconsin Federation of Women's Clubs. Numerous women historians were members of the American Association of University Women and conducted historical and sociological studies of college women and the contemporary issues that concerned them.[36]

Many were active in women's rights organizations and were concerned with woman suffrage, the Equal Rights Amendment, temperance, child welfare, labor, war, and peace—issues that might be considered of special interest to women—but that affected all of society. Their struggles for equality in the larger world informed their struggle within the historical profession. Several, including Lucy Salmon and Mary Williams, viewed their work in women's history and for women's equality in the public sphere as directly linked. Salmon participated in the College Equal Suffrage League at Vassar but advocated that women be given suffrage because it would benefit everyone, not just women. Both a student of and participant in the movement for women's rights, Williams used her historical skills in lobbying campaigns for passage of an Equal Rights Amendment. Some women served as delegates to the Women's Centennial Congress called by Carrie Chapman Catt in 1940 to commemorate the "Woman's Century." The congress celebrated the achievements of American women with sessions on women's history and plans for future political activity among women. Emily Hickman, of the New Jersey College for Women, and president of the Berkshire Conference of Women Historians in 1940, served as a member of the National Organizing Committee and was a delegate to the Centennial Congress. Several others—Viola F. Barnes of Mount Holyoke, Susan M. Kingsbury of Bryn Mawr, Jeannette P. Nichols, then unemployed, and Dorothy Stimson of Goucher—were also invited

[36] Jane Newell to Louise Phelps Kellogg, *ca.* 1918, Mrs. Henry Wade Rogers to Kellogg, May 13, 1919, Theodora W. Youmans to Kellogg, October 20, 1919, and October 26, 1921, Mrs. Herbert Main to Kellogg, August 27, 1923, Kellogg to Main, September 8, 1926, Gertrude L. Murray to Kellogg, May 11, 1932, all in Kellogg Papers; Mary W. Williams to James A. Robertson, July 11, 1922, Robertson Papers; Williams to Jameson, May 9, 1926, JFJ Papers; Williams to Mabel Vernon, April 15, 1931, and November 19, 1933, and Vernon to Williams, November 21, 1933, all in records of the Women's International League for Peace and Freedom, American Section, Peace Collection, Swarthmore College; Edith Houghton Hooker to Martha Souder, January 16, 1937, Williams to Souder, January 23, 1937, Hooker to Mrs. West, October 14, 1937, Williams to Souder, March 16, 1938, Caroline L. Babcock to Mrs. Leigh, March 20, 1939, all in NWP Records. Among the others active in the American Association of University Women were were Alice Freeman Palmer, Lucy Salmon, Susan M. Kingsbury, Ella Lonn, and Lois Mathews (Rosenberry), who with Marion Talbot, sociologist and AAUW founder, published a history of the organization in 1931. See Ella Lonn, "Academic Status of Women on University Faculties," *Journal of the American Association of University Women*, 17 (January 1924): 5–11; Lonn, "The Work of Recognition," *JAAUW*, 20 (January 1927): 33–36; Talbot and Rosenberry, *The History of the American Association of University Women, 1881–1931* (Boston, 1931), 9, 10, 32, 43, 63, 71, 104, 114, 133, 134, 199, 200, 230–33, and 313. Also see Nancy F. Cott, *A Woman Making History: Mary Ritter Beard through Her Letters* (New Haven, Conn., 1991), on Beard's World Center for Women's Archives.

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delegates at the congress.[37] Their work in women's organizations was part of a larger commitment to reform activities. To help local citizenry make informed judgments on contemporary reform movements, these women considered education in the history of similar movements in the past essential.[38] Concerned with social reform movements, this small group of activist women historians also led the effort to reform their own profession, and they were joined by numerous other women and some liberal male historians.

ALTHOUGH COMPRISING ABOUT 15 TO 20 PERCENT of the members of the AHA from its founding until 1940, women never garnered a similar percentage of representation on AHA governing bodies, committees, or on programs at annual meetings. While most annual meetings included at least one and sometimes several papers presented by women, some years saw no women on the program at all. This was the case in 1919. After returning from this meeting, Mary Williams expressed her disgust at the situation in a letter to James A. Robertson:

> All that any twentieth century woman of sense asks is a fair field and no favors;—to her, chivalry merely means a "square deal,"—but the woman who is so unfortunate as to have specialized in history largely asks in vain.—Witness the program of the A. H. A. convention. Not a woman's name appears upon it; and yet you know and I know that there are at least a dozen women in the country who could present papers equal if not superior to those given by some of the "two-for-a-penny" men who are listed. Most of the influential men of the A. H. A. stand tight, shoulder to shoulder, with both fore-feet in the trough.[39]

Although Williams was much more outspoken than most of her colleagues, other women had complained about the low level of participation by women in the annual meeting program. Part of the problem was that women did not aggressively promote themselves, or their work, were reluctant to offer to give papers, and needed to be asked to participate.[40] Henry E. Bourne, who taught in the woman's college of Western Reserve University and chaired the 1916 program committee, recognized that women needed to be asked to participate and

[37] Louise Fargo Brown, *Apostle of Democracy: The Life of Lucy Maynard Salmon* (New York, 1943), 256; Jacqueline Goggin, "Mary Wilhelmine Williams: Feminist Activist and Historian" (Unpublished paper presented at the Eighth Berkshire Conference on the History of Women, Douglass College, June 1990, and at the Bunting Institute Colloquia Series, Radcliffe College, January 1991, in author's possession); and Women's Centennial Congress, "Declaration of Purpose" (1940), and list of delegates to the congress, November 25–27, 1940, Catt Papers.

[38] See, for example, Lillian W. Johnson to A. Howard Clark, February 7, 1903, AHA Records; Kathryn Abby to James A. Robertson, November 20, 1929, Mary W. Williams to Robertson, September 5, 1932, Robertson to Williams, October 28, 1932, Williams to Robertson, November 1, 1932, Abby to Robertson, November 21, 1932, and January 26, 1935, all in Robertson Papers; Kathleen Bruce to Merle Curti, April 29, 1936, Curti Papers; Ella Lonn, "Issues before the Maryland Electorate," *Baltimore News*, November 3, 4, and 5, 1923; Lonn, "Women's Colleges and Americanization," *Bulletin of Goucher College*, 7 (April 1920).

[39] Williams to James A. Robertson, December 30, 1919, Robertson Papers; Hesseltine and Kaplan, "Women Doctors of Philosophy in History," 254–59; Arthur S. Link, "The American Historical Association, 1884–1984: Retrospect and Prospect," *AHR*, 90 (February 1985): 1–17; and "Appendix C—Decennial Check of Women Participants in AHA programs and on AHA committees," Rose Report, 1970, AHA Records.

[40] Susan M. Kingsbury to Charles H. Haskins, October 17, 1906; Nellie Neilson to Haskins, October 20, 1912, Lucy Salmon to Evarts B. Greene, December 16, 1916, December 15, 1917, March 5, 1919, and March 21, 1919, all in AHA Records.

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FIGURE 4: Elizabeth Donnan, 1934, photo by Bachrach. Courtesy of the Margaret Clapp Library, Wellesley College.

proposed a special session in which women could "show what they could do." Although Bourne had in mind a paper such as "What Women Have Done in American Historical Writing," the program committee could not agree on the proposal. Some members thought it a good idea, while others remarked "that it savors of segregation and would be offensive to the women." Since no women were on the program committee, Bourne sought advice from Elizabeth Donnan, a professionally trained economist who wrote economic history and worked on Jameson's staff at the Carnegie Institution's Department of Historical Research. Donnan did not think much of Bourne's idea, maintaining that women wished to be judged only by the quality of their work.[41]

[41] Henry Bourne to Jameson, April 3, 1916, Jameson to Bourne, April 12, 1916, both in JFJ Papers.

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Female participation on annual meeting sessions usually increased when women were appointed to program committees. Thus a strategy adopted by activist women during the 1920s and 1930s was to advocate the appointment of at least one woman to the program committee to ensure that women would be asked to present papers. This was the case, for example, in 1925, when William E. Dodd chaired the program committee and Annie Heloise Abel-Henderson served as a member. Both made a concerted effort to obtain female participants, and six women presented papers at that meeting.[42]

Since AHA program committees not only selected the participants but shaped the content of sessions, few papers on the history of women were presented. In 1940, Jeannette P. Nichols was researching her AHA paper and wrote to Thomas P. Martin of the Library of Congress seeking access to the papers of Harriot Stanton Blatch. Nichols made a case for the importance of her research: "For the first time in the history of the American Historical Association, there is to be a session devoted to the activities of women."[43] It was indeed the first time in fifty-five years that an entire session on women's history was presented at an AHA meeting. Mildred Thompson served as chair, Nichols presented a paper on American feminism, with Alma Lutz as commentator, and Lillian E. Fisher spoke on the status of women after the Mexican Revolution. Vera Brown Holmes offered comments. Another first at the 1940 meeting was an entire session on black history with papers presented by black historians. Mary Williams served on the 1940 program committee chaired by Merle Curti and was responsible for organizing these two sessions.[44]

Although women were not often invited to participate in the annual meeting of the AHA, usually more than one hundred women attended; not only historians but wives of male historians, female high school teachers, and women interested in history who were members of patriotic societies such as the Colonial Dames and the Daughters of the American Revolution (DAR). Aside from formal program sessions, AHA meetings also included a variety of luncheons, dinners, and smokers. Because few women historians regularly attended AHA meetings in the early years, male historians established a "Committee for the Entertainment of the

[42] Kathleen Bruce to Dodd, January 14, 1925, Abel-Henderson to Dodd, March 20, 1925, Elizabeth Baldwin to Dodd, March 28, 1925, Dorothy Dondoree to Dodd, April 6, 1925, Dodd to Dondoree, April 24, 1925, Dorothy Louise Mackay to Dodd, April 27, 1925, Dodd to Abel-Henderson, May 5, 1925, Ella Lonn to Dodd, September 2, 1925, copy of final program for 1925 meeting, all in Dodd Papers.

[43] Jeannette P. Nichols to Thomas P. Martin, November 12, 1940, LC Archives; *Annual Report of the American Historical Association for the Year 1940* (Washington, D.C., 1940), 21. In previous years, two papers on women were presented at AHA meetings. In 1924, Susan H. Ballou of Bryn Mawr presented "Women in Industrial Life in Ptolematic Egypt," and Thomas P. Abernathy of the University of Virginia presented "The Changing Status of Women in the Ante-Bellum South," at a session on the Old South at the 1933 meeting. See copy of 1924 AHA Program in Albert J. Beveridge Papers, Manuscript Division, Library of Congress, and copy of the 1933 program in AHA Records.

[44] Williams to W. E. B. Du Bois, January 24, 1940, Du Bois to Williams, February 1, 1940, Williams to Du Bois, February 11, 1940, Du Bois to Williams, February 20, 1940, Williams to Du Bois, April 9, 1940, all in W. E. B. Du Bois Papers, University of Massachusetts at Amherst; Dorothy M. Quynn to Mary W. Williams, January 4, 1940, Louise Fargo Brown to Williams, January 10, 1940, Howard K. Beale to Williams, February 13, 1940, all in Williams Papers. Also see *Annual Report of the American Historical Association for the Year 1940*, 15; Cott, *Woman Making History*, 222–23. Mary Beard apparently was asked to chair the session on women but declined since she had no role in setting it up. She provided Nichols with some material on women but believed that Nichols did not use it.

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Ladies," which planned activities for all the women attending. In its formative years, the AHA often met jointly with the American Political Science Association and the American Economic Association, and women in these professions were also invited to the sexually segregated social functions.[45]

As early as 1905, Lucy Salmon suggested that the "Committee on Social Entertainment of the Ladies" be abolished, contending that the women with whom she had talked objected to it: "We do not care for afternoon teas where we meet society women, and deprecate entertainments that separate the members into two classes, men and women."[46] While other women might have agreed with Salmon, few wrote letters that expressed similar sentiments in the early years. The men of the AHA not only desired separate social functions, they also believed the majority of women did, too; and men were more comfortable without a female presence that inhibited their smoking and use of coarse language. J. Franklin Jameson viewed women who were bothered by smokers as "complainers." At the 1908 AHA meeting, it was proposed that smokers and sessions be combined. Jameson, who "did not find the feminine element particularly useful at . . . meetings" felt this was excessive. He acknowledged that women had to be "reckoned with. We have to treat them well," making sure they "do not think that we are treating them particularly well," since many "value absolute equality more than all other earthly blessings."[47] Even if prevailing sentiments had dictated a different course, male historians continued to hold luncheons, dinners, and smokers in men's clubs in host cities, thereby ensuring that women would be excluded.

Beginning in the late 1910s, however, when more women historians began to attend annual AHA meetings, some became vocal in their opposition to sexually segregated social functions. Their complaints led local arrangements committees to solicit advice from a few women historians. To their dismay, male historians found that there was no consensus among the women and, only partly in jest, began to call the controversy the "Woman Question."[48] DAR member Mary Lockwood also objected to the special dinners held for women and threatened to

[45] Wilbert Siebert to Patty W. Washington (Assistant Secretary of the AHA), February 12, 1923, Washington to Siebert, February 17, 1923, both in AHA Records. Washington maintained that in prior years the average attendance of women at AHA meetings was about 150. On smokers at scientific meetings and women's responses to them, see Rossiter, *Women Scientists in America*, 91–95. Similar problems continue to plague professional women. In "Women at Work," *New York Review of Books* (August 14, 1986): 26–36, Andrew Hacker cited an article in the March 24, 1986, issue of the *Wall Street Journal*, "The Glass Ceiling," noting that the biggest obstacle to women's advancement in the corporate world is that a great deal of business takes place in virtually all-male preserves—golf courses, squash clubs, and tennis courts.

[46] Lucy Salmon, signed nomination form, presumably sent to all members of the AHA, undated but filed with 1905 records, AHA Records.

[47] Quotation is from Jameson to Waldo G. Leland, August 27, 1908, JFJ Papers. Also see Walter Francis Wilcox (of the American Economic Association) to Waldo G. Leland, November 11, 1915, Jameson to I.J. Cox, January 3, 1916, Leland to Mrs. William B. Trott, January 6, 1916, Evarts B. Greene to Wilcox, November 8, 1916, all in AHA Records; Max Farrand to Jameson, October 25, 1922, Jameson to Fairfax Harrison, December 13, 1927, both in JFJ Papers; Mary Williams to William A. Frayer, January 6, 1925, and Williams to William E. Dodd, no day, January 1925, both in Dodd Papers. After 1927, there is little correspondence pertaining to smokers, but an examination of annual meeting programs in Jameson's Papers through 1936 reveals that they continued to be held.

[48] William E. Lingelbach summarized the conflicting advice he received in a letter to Waldo Leland, March 18, 1918, AHA Records.

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resign from the AHA if the practice continued. In 1916, she wrote to Clarence Bowen, "Why should the women be sent off to a hotel by themselves for dinner . . . To be sure there was *one* woman on the programme! (Thanks for that!)"[49] Apparently, Bowen forwarded her letter to AHA secretary Evarts B. Greene, who replied that he believed that most women preferred to have separate social functions at AHA meetings.

In fact, women historians lined up on both sides of the issue. Their perspective depended on whether and where they taught. Women like Lucy Salmon and Mary Williams, who taught in women's colleges and had mostly female colleagues, demanded absolute equality when they participated in the larger professional arena. They recognized that AHA politics were discussed and information about jobs, fellowships, and research exchanged at these sexually segregated social functions. They argued that women's exclusion hindered them professionally. At the 1917 annual meeting, women were permitted to attend the smoker for the first time. Williams wrote to the chair of the 1918 local arrangements committee urging that the practice be continued. While she "loathe[d] the 'weed' in all of its forms," she enjoyed herself and "met many . . . men friends in the guild and made new acquaintances among the men, which would not have been possible but for the gathering." She added that she believed the "opportunity for social and intellectual exchange to be good for men and women alike."[50]

While women in women's colleges believed it was more valuable to meet and talk with men at an AHA meeting, women outside of academe and those who taught in coeducational colleges and universities, especially those in the Midwest, had few female colleagues. They desired greater interaction with other women historians and thus were in favor of maintaining separate social functions for women at AHA meetings. One woman who taught in a coeducational midwestern college wrote to the local arrangements committee in 1918: "Are those eastern women so stupid that they can't see that a closer association of all of us would help, not hinder the common cause?" She went on to voice her displeasure at the fact that the few women who were recognized by the men of the AHA and served on committees tended to come from the Seven Sisters colleges.[51]

HER COMPLAINTS WERE NOT UNFOUNDED, for Lucy Salmon served as the token female member on several AHA committees during the first thirty-five years of its existence. She was also the first woman to be elected to the Executive Council in 1915, after a few liberal men in the AHA waged a two-year campaign to get her elected. Although Salmon had devoted years of service to the AHA, with

[49] Mary K. Lockwood to Clarence Bowen, December 11, 1916 (emphasis in original), Evarts B. Greene to Lockwood, December 16, 1916, both in AHA Records. Lockwood also was active in the District of Columbia Federation of Women's Clubs, and her correspondence is in the archives of the General Federation of Women's Clubs, Washington, D.C.

[50] Williams to William E. Lingelbach, December 30, 1917, Lucy Salmon to Evarts B. Greene, December 16, 1916, Salmon to Greene, December 15, 1917, all in AHA Records.

[51] Unidentified woman historian quoted in Lingelbach to Leland, March 18, 1918, AHA Records. Some men also may have favored the Seven Sisters because they sent their daughters there. Katrina Jameson, the daughter of J. Franklin Jameson, for example, began her college career at Smith.

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appearances on annual programs, and appointments to the General Committee, the Membership Committee, the Committee of Seven (concerned with history in secondary schools), and was the only woman historian at the founding meeting of the *American Historical Review*, there were objections to her candidacy for the Executive Council.[52] Salmon had nominated Ruth Putnam, an expert on the Netherlands who was not professionally trained, for the Executive Council in 1905, but her male colleagues would not support Putnam. It took nine years and continued protests from Salmon and other women before the men in the AHA would consider a woman candidate seriously.[53]

Lois Mathews of the University of Wisconsin served on the 1914 nominating committee and urged her male colleagues to place a woman on the Executive Council ballot. Although Charles Hull, chair of the committee, viewed Mathews' advocacy of a woman candidate and continued protests by women as "political feminism," he sought advice regarding suitable female candidates from leading men in the AHA. Women historians' "political feminism" contributed to the admission that although "[t]heoretic considerations as to equality or proportional representation" favored a woman's candidacy, J. Franklin Jameson, for one, "could not think of one whose advice on the Council" he desired. "If there were some woman that I thought better qualified for a seat on the Council than any of the men I have named, I should name her," Jameson contended. If a woman had to be nominated, he favored Ruth Putnam over Lucy Salmon.[54] Evarts B. Greene, Columbia University professor and AHA secretary, wanted a woman considered for nomination and sent a list of potential candidates to leading men in the AHA. Carl Russell Fish favored a woman but expressed no preference as to individuals. Sidney B. Fay favored Ruth Putnam or Nellie Neilson, and Herbert E. Foster also suggested Neilson.[55]

Yet no woman was nominated for the council in 1914. Lois Mathews also served on the 1915 nominating committee, chaired by Charles Rammelkamp, and she again raised the issue of a woman's candidacy. While Rammelkamp feared that "the presence of a woman on the council might change somewhat the social feature of the council or committee meetings," he asked AHA secretary Greene if there were "any reasons . . . why a woman should not be nominated and elected to the council?" Greene was "quite in sympathy with the desire of women who are members of the Association, to have such representation" and suggested Ruth

[52] J. Franklin Jameson, "The American Historical Review, 1895–1920," *AHR*, 26 (October 1920): 6; Brown, *Apostle of Democracy*; Herbert Baxter Adams to H. Morse Stephens, January 19, 1897, Lillian W. Johnson to Charles H. Haskins, December 12, 1902, Salmon to Leland, June 11, 1912, all in AHA Records. For Salmon's presentations at annual meetings, see programs for 1885, 1897, 1901, 1903, 1905, 1907–10.

[53] Salmon, signed nomination form, undated, filed with 1905 records, and Mrs. Jane Bancroft Robinson to Charles H. Haskins, December 1, 1905, both in AHA Records. Jane Bancroft (Mrs. George D. Robinson) received the Ph.D. in history from Syracuse University. On Putnam, see Charlotte Williams Conable, *Women at Cornell: The Myth of Equal Education* (Ithaca, N.Y., 1977), 85, 90.

[54] Jameson to Charles H. Hull, August 24, 1914, JFJ Papers; Hull to Evarts B. Greene, November 8, 1914, AHA Records; and "The Meeting of the American Historical Association at Charleston and Columbia," *AHR*, 19 (1913–1914): 491.

[55] Charles H. Hull to Greene, October 16, 1914, and enclosure, "Appendix of Suggestions Received"; Greene to Sidney B. Fay, October 27, 1914, Fay to Greene, November 8, 1914, Herbert E. Foster to Greene, November 3, 1914, all in AHA Records.

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FIGURE 5: Nellie Neilson, n.d. [1943]. Courtesy of Mount Holyoke College Archives.

Putnam, Bertha Putnam, and Nellie Neilson.[56] Jameson was consulted and again suggested Ruth Putnam but added that "men have better chances of knowing who is who and what is what in the historical profession."[57]

[56] Charles Rammelkamp to Greene, November 18, 1915, Greene to Rammelkamp, November 19, 1915, Trustees Correspondence, files of the Illinois State Historical Library, University of Illinois, Champaign-Urbana.

[57] Jameson to Charles H. McIlwain, September 6, 1915, AHA Records. Jameson also noted that

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FIGURE 6: Bertha Putnam, *circa* 1937. Courtesy of Mount Holyoke College Archives.

Despite some men's objections, Lucy Salmon was nominated and elected to the Executive Council. She encountered unequal treatment during her term. In 1916 and 1917, the council met for two days in New York City prior to the annual

men in women's colleges did not get their share of positions within the AHA. Also see "The Meeting of the American Historical Association in Chicago," *AHR*, 20 (1914–15): 526. Bertha Putnam was the niece of Ruth Putman and taught at Mount Holyoke with Neilson. See Glazer and Slater, *Unequal Colleagues*, 25–68.

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meeting. The other council members arranged to hold a dinner at the Metropolitan Club, which denied access to women. Salmon both times contended that she had other plans for dinner, but probably she was embarrassed and uncomfortable. Only a few of her male colleagues expressed reservations at her exclusion, and discussion of council business continued without her.[58]

Undaunted by the insensitivity of her male colleagues, Salmon was a productive council member and whenever possible proposed women for AHA committees and program sessions. While she was "reluctant to suggest any radical change in policy," Salmon continued, as she had in the past, to promote female candidates. Salmon knew personally the women she recommended, most of them colleagues from the Seven Sisters colleges, and in letters to her fellow council members, she took care to provide ample information about the women she recommended, including their research and teaching specializations. Salmon achieved modest success; when she left the council in 1920, there were four other women historians serving on AHA committees.[59]

When Salmon was elected, she succeeded in breaking the sex barrier. For the next twenty-five years, women served on the Executive Council. When Salmon left office, Ruth Putnam was elected, followed by Mary Williams in 1922. Williams's candidacy and election were part of a movement orchestrated by midwestern women who argued that historians from the Seven Sisters colleges did not adequately represent their interests in the AHA. Shirley Farr confided to her University of Chicago colleague William E. Dodd that she had the "temerity" to nominate Cleo Hearon of Agnes Scott College for the council, even though "she hasn't begun as scullery maid and worked up to chief-lady-of-the-clothes-press, and thus by gradual stages by forty years service to the top." What moved her to nominate Hearon was the "naive assumption" that Ruth Putnam had represented women's interests in the AHA. You "might as well say that W. R. Hearst was competent to represent the readers of the Atlantic," Farr asserted, for "he is a human being and so are they, but the likeness ceases there."[60] Nor, in Farr's view, did Lucy Salmon represent all the women of the AHA. Although Hearon was not nominated, Farr's objection and those of other women in the Midwest may have led to Williams's nomination and election. While she also came from a women's college in the east, she was outside the Seven Sisters and had established a reputation as an outspoken critic of discrimination against women.[61]

[58] Salmon to Leland, January 4, 1916, Evarts B. Greene to William A. Dunning, September 13, 1916, Dunning to Greene, September 18, 1916, Greene to Dunning, October 18, 1916, Dunning to Greene, October 24, 1916, Greene to Salmon, October 6, 1917, Clarence Bowen to Greene, October 18, 1917, Greene to Worthington C. Ford, October 23, 1917, George Lincoln Burr to Greene, October 23, 1917, Salmon to Ford, October 26, 1917, Greene to Ford, October 30, 1917, all in AHA Records.

[59] Salmon to Evarts B. Greene, December 16, 1916, December 15, 1917, December 18, 1918, March 5, 1919, March 21, 1919, and April 30, 1919, Greene to Salmon, May 8, 1919, Salmon to Greene, May 11, 1919, and November 25, 1919, all in AHA Records. Also see "The Meeting of the American Historical Association at Cleveland," *AHR*, 25 (1919–20): 389–90. In 1920, Eloise Ellery was on the nominating committee, Nellie Neilson was on the Herbert Baxter Adams Prize Committee, and Julia S. Orvis and Louise Fargo Brown were on the Membership Committee.

[60] Shirley Farr to William E. Dodd, September 23, 1922, Dodd Papers; "Meeting of the American Historical Association in Cleveland," 389.

[61] Like their male colleagues, women also might have been reacting more generally against the

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Williams lived up to her reputation at her first council meeting, much to the dismay of Jameson, who believed that the council had "landed a lemon." Williams immediately assumed "the role of champion of the oppressed, [and] she began . . . to work up an agitation for more females on the programme."[62] Like Salmon, Williams also suggested women for AHA committees, annual meeting sessions, and she protested continuously against segregated social functions at annual meetings. When she left office in 1926, Williams suggested several women to replace her. Among them were Frances G. Davenport of Jameson's staff in the Carnegie Institution's Department of Historical Research, Violet Barbour of Vassar, and Nellie Neilson.[63]

Although Neilson reportedly had been in poor health, she was elected to replace Williams. Neilson was followed by Elizabeth Donnan, then teaching economics at Wellesley. While women outside the Seven Sisters colleges held a few appointments on AHA committees, it was only after Donnan's term ended in 1936 that a midwesterner from a coeducational institution was elected to the council. Bessie L. Pierce from the University of Chicago served until 1940, when she was replaced by a man.[64] Women's representation on AHA governing bodies and committees actually decreased in the decades after 1940, in spite of the fact that during the 1930s women began to take collective action to ensure that their interests were represented in the AHA.[65]

While women historians outside the academy and those who taught in midwestern coeducational institutions had long advocated women organizing to promote their interests in the AHA, it was only in the 1930s that collective action was taken. The generation of women who began their careers in the mid-1910s and early 1920s were more actively feminist than their older counterparts. In 1930, some of these women decided to organize a regional professional support group for women historians. Comprised mainly of women from the Seven Sisters,

eastern/New England regional bias in all of the AHA's governing bodies. See Novick, *That Noble Dream*, 181. An anonymous reviewer for the *AHR* correctly pointed out to me that Goucher is sometimes referred to as the "Eighth Sister," and thus Williams was not really an outsider. At this point, I do not have any additional data on this subject from midwestern women and can only speculate that Williams was such a maverick that she was more acceptable.

[62] Jameson to Leland, January 9, 1923, JFJ Papers. In this same letter, Jameson also reported that "[Williams] told Miss Putnam, retiring member, aggressively, that now it was time to proceed to 'force things through' in this matter. Miss Putnam replied that the best course for a new councillor was in her first year to keep quiet." Clearly, Jameson preferred the passive behavior of Putnam over that of Williams.

[63] Williams to William A. Frayer, January 6, 1925, Williams to Dodd, January 1925, Annie Heloise Abel-Henderson to Dodd, 20 March 1925, all in Dodd Papers; Williams to H. B. Learned, June 7, 1926, AHA Records.

[64] See Patty W. Washington to Neilson, January 3, 1927, AHA Records; "The Meeting of the American Historical Association at Rochester," *AHR*, 32 (1926–27): 952; "Boston Meeting of the American Historical Association," *AHR*, 36 (1930–31): 512; "Historical News: American Historical Association," *AHR*, 42 (1936–37): 622. Lillian W. Johnson, from the University of Tennessee at Knoxville, was a member of the AHA's General Committee in 1903. See Johnson to A. Howard Clark, February 7, 1903, AHA Records. Frances G. Davenport, of Jameson's staff, served on the 1927 local arrangements committee. See Jameson to Fairfax Harrison, December 13, 1927, JFJ Papers. Louise Phelps Kellogg, of the State Historical Society of Wisconsin, served on the 1933 and 1934 nominating committees.

[65] "Appendix C," and Rose Report, 1970, AHA Records; Link, "American Historical Association," 5.

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as well as from women's colleges in upstate New York and southern New Jersey, the Lakeville Conference promoted the interests of women in the AHA.

THE LAKEVILLE CONFERENCE was organized in part because women were excluded from similar regional groups formed by male historians. Each fall since the mid-1920s, Jameson had been holding a retreat, or "historical convivium," as he called it, for male historians in Branford, Connecticut. While the convivium served mainly as a social gathering place for the elder statesmen of the profession, Jameson often carefully selected and invited a few younger male colleagues whose exposure to their elders would benefit their careers. Assuming incorrectly that women historians would be invited, Mary Williams wrote to AHA secretary Dexter Perkins for more information about the convivium and wondered if an announcement would be published in the *AHR*.[66]

Younger male historians from New York State, many of whom were not among the privileged group invited to Jameson's Branford gatherings, nevertheless liked the idea of holding an exclusive social retreat and held their own first meeting in May 1929. In promotional materials, the organizers announced, "There is no intention to overlook anybody." Their purpose was to attain "closer relations among history men in New York State," which they meant literally, for women were excluded.[67] Louise Fargo Brown, from Vassar College, recalled in 1951 that several of the men in the Cazenovia, New York, group told her "frankly that the informality, one of the most valued features of the meetings, would inevitably disappear if women were asked to attend."[68]

Returning on the train together after the 1929 AHA meeting, Brown and Louise Ropes Loomis of Wells College began discussing their exclusion from the Cazenovia group and decided to solicit advice from other women historians regarding the formation of a similar group for women. In the next few months, they wrote to women throughout the region and received enthusiastic support for the idea. Their first meeting was held in May 1930 in Lakeville, Connecticut, and thus the group was called the Lakeville Conference until 1936, when the name was changed to the Berkshire Conference of Women Historians.[69]

[66] There are scattered letters to male historians on the historical convivium in Jameson's papers. Merle Curti was among those younger scholars whom Jameson invited to the annual gathering in the 1920s and 1930s, and he described his experiences in an interview with me. Goggin, interview with Curti, June 21, 1985, Madison, Wisconsin; Williams to Dexter Perkins, June 3, 1928, AHA Records. Unfortunately, Perkins' reply to Williams has not been located.

[67] "Cazenovia History Conference," undated promotional announcement, *ca.* 1929, in JFJ Papers.

[68] Louise Fargo Brown, statement on the founding of the Berkshire Conference, May 1951, Berkshire Conference Records, Schlesinger Library, Radcliffe College.

[69] Brown, statement, May 1951, Berkshire Conference Records. Also see the brief history of the organization introducing the finding aid to the records of the Berkshire Conference. The group was called the Lakeville Conference until 1936, when the name was changed to the Berkshire Conference of Women Historians. Although no membership lists exist in the records of the Berkshire Conference, a collective biography of thirty-three organizers, members, and their supporters outside the region was constructed. Three major sources, the Berkshire Conference Records, AHA Records, and Margaret A. Judson, "The Berkshire Historical Conference," in *Breaking the Barrier: A Professional Autobiography by a Woman Educator and Historian before the Women's Movement* (New Brunswick, N.J., 1984), 77–87, yielded the following list: Mary Albertson, Violet Barbour, Viola F. Barnes, Mary Benson, Louise Fargo Brown, Vera L. Brown (Holmes), Meribeth Cameron, Mildred

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Just nine months after they organized, these women began to discuss the establishment of an exchange system whereby conference members would exchange jobs for an academic year. Because their employment prospects were limited, and they could become stale and intellectually isolated when confined in the same job over a period of years, even the prospect of a year of teaching in another women's college in the region was appealing. Women in the conference were polled, and more than thirty-five expressed interest in the job exchange. Financial and administrative details could not be worked out, however, and the exchange system as it was originally conceived was not adopted. Yet women in the conference suggested that other conference members be asked to replace them when they went on sabbatical, and thus some individual women benefited from a year of teaching at another women's college.[70]

By the late 1930s, women in the Berkshire Conference began to see the necessity of influencing AHA policy through collective action. Louise Fargo Brown was invited to present a paper on a series of special panels on historiography at the 1937 annual meeting, but, when it was decided to publish the proceedings of these panels, Brown's paper was not included. She wrote a letter of protest to Conyers Read, the executive secretary, who assured her that her paper had been given serious consideration, for it was "feared that old cry would be raised of discrimination against the sex." Read claimed that, although the program committee made a deliberate attempt to invite more women to present papers at these special sessions, they "were aiming in each case at the most distinguished American scholars in their particular bailiwicks." Read then sug-

Campbell, Evalyn Clark, Gerda Richards Crosby, Edith Farnham, Dorothy Ganfield Fowler, Leona Gabel, Mary Gambrell, Amy Gilbert, Emily Hickman, Elizabeth Hodder, Beatrice Hyslop, Margaret A. Judson, Louise Phelps Kellogg, Elisabeth G. Kimball, Louise Ropes Loomis, Dorothy Louise Mackay (Quynn), Anne Bush Maclear, Helen Taft Manning, Lois Merk, Wilma Pugh, Frances Relf, Jane Ruby, Dorothy Stimson, Judith Blow Williams, Mary Wilhelmine Williams, and Helen Louise Young.

Several women moved from colleges in the Berkshire region to midwestern institutions, or from midwestern institutions to those in the Berkshire region during the 1930s. Thus they were able to broaden the feminist network of women historians beyond New England and the mid-Atlantic states. Although Kellogg, Quynn, and Mary Williams were not members of the Lakeville Conference, they have been included with this group because of their roles in the campaign to elect Nellie Neilson to the AHA presidency and because they corresponded with conference members. Stimson, who taught at Goucher with Williams, did attend conference meetings, according to Judson. Almost half of the group received their bachelor's degrees at women's colleges between 1895 and 1920, during a period of heightened feminist activity among college women, and most received the doctorate from Ivy League institutions. On networking among women, see Estelle Freedman, "Separatism as Strategy: Female Institution Building and American Feminism, 1870–1930," *Feminist Studies*, 3 (Fall 1979): 512–29; Susan Ware, *Beyond Suffrage: Women in the New Deal* (Cambridge, Mass., 1981); and Ware, *Partner and I: Molly Dewson, Feminism, and New Deal Politics* (New Haven, Conn., 1987).

[70] Vera L. Brown to Helen Allen, March 3, 1931, Louise Fargo Brown to Vera L. Brown, March 8, 1931, both in Berkshire Conference Records. Brown reported that Mary Williams "want[ed] to arrange a regional meeting similar to ours." Vera L. Brown, Louise Ropes Loomis, and Louise Fargo Brown, form letter, March 16, 1931, and enclosed questionnaire on faculty exchange; "Extracts from Secretary's Report," on responses to questionnaire, presented at annual meeting, May 16, 1931; Viola Barnes, for example, spent a semester at Smith College, see "Lakeville History Group," Report of the Secretary, 1934, all in Berkshire Conference Records; Report of the 1936 Meeting in "Historical News," *AHR*, 42 (1936–37): 196. The exchange system continued to be discussed through 1938. See "Berkshire Historical Conference, Secretary's Report," 1938–39, Berkshire Conference Records. Also see Judson, *Breaking the Barrier*, 77–87.

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gested that Brown send him a list of women historians and their fields of specialization for consideration by future AHA program committees. Brown acted on this suggestion and, at the 1938 meeting of the Berkshire Conference, asked those in attendance to compile and send to Read lists of women and their specializations.[71]

Women in the Berkshire Conference began to nominate and vote on candidates whom they would support collectively for election to AHA governing bodies and committees. Once the conference agreed on a slate of candidates, individual members submitted the names of their candidates to the AHA nominating committees and engaged in bloc voting. This strategy was successful, and by 1940, Nellie Neilson was finally elected as second vice president of the AHA.[72]

The campaign to elect Neilson had actually begun in 1932. At the annual meeting that year, Mary Williams began promoting Neilson's candidacy among the women in attendance. Although the two women may never have met, Williams undoubtedly believed that Neilson was the strongest female candidate. Discussions among women at the 1932 AHA meeting resulted in Neilson emerging as the favored name. She also may have had enough support among male historians to be a viable candidate. Perhaps Williams and other women heard of plans to promote Harvard professor Charles McIlwaine, another distinguished medievalist, for the presidency and believed that Neilson was a superior scholar.[73]

Neilson's scholarly credentials certainly equalled McIlwaine's. She had been teaching at Mount Holyoke College since 1902 and was promoted to full professor in 1905. The best known of the few women who had received the doctorate before 1900, Neilson was the first woman to publish in the *AHR*; her article appeared in 1897, two years before she completed her degree. Working under the direction of Charles McLean Andrews at Bryn Mawr, Neilson also studied at the London School of Economics with Paul Vinogradoff and Frederick William Maitland, pioneers in the field of British medieval history. She achieved international recognition as a member of the Selden Society and the Institute of Historical Research, was a founding member of the Medieval Academy of America and its journal *Speculum*, was elected president, and for many years was the only female fellow. The British Royal Historical Society also named her a fellow. The 1920s were Neilson's most productive years; she published three edited collections of documentary source materials and two pathbreaking articles. Active in the AHA since its early years, Neilson had served on several committees as well as the Executive Council and, more important, had earned the respect of men in her field, who recognized her superior scholarship and teaching skills.[74]

[71] Conyers Read to Louise Fargo Brown, February 28, 1938, Read to Brown, February 8, 1938, Brown to Read, February 19, 1938, all in AHA Records; Berkshire Historical Conference, Report of the Secretary, 1938–39, Berkshire Conference Records.

[72] Report of the Secretary, 1938–39, Emily Hickman and Beatrice Reynolds, form letter to members, June 9, 1939, Hickman and Dorothy Canfield Fowler, form letter to members, May 23, 1940, Fowler, form letter to members, June 9, 1940, Report of the Secretary, 1940–41, Fowler to Lawrence B. Packard, November 3, 1940, all in Berkshire Conference Records.

[73] Williams to Conyers Read, April 4, 1933, and April 5, 1933, both in AHA Records.

[74] See Margaret Hastings and Elisabeth G. Kimball, "Two Distinquished Medievalists—Nellie Neilson and Bertha Putnam," *Journal of British Studies*, 18 (Spring 1979): 143–59; Glazer and Slater,

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FIGURE 7: Nellie Neilson, n.d. Courtesy of Mount Holyoke College Archives.

In the spring of 1933, Williams orchestrated a letter-writing campaign for Neilson. She wrote to about twenty women historians across the country, informed them of the campaign to elect Neilson as second vice president and

Unequal Colleagues, 25–68; Neilson's obituary in the *AHR*, 53 (October 1947): 219–20; Kathryn Kish Sklar, "American Female Historians in Context, 1770–1930," *Feminist Studies*, 3 (Fall 1975): 179–80; Charles McLean Andrews to Jameson, June 3, 1895, July 31, 1895, November 23, 1895, October 16, 1896, November 30, 1896, and January 20, 1897, all in JFJ Papers; Charles Haskins to Neilson, October 12, 1912, Neilson to Haskins, October 20, 1912, Haskins to Neilson, October 21, 1912, Neilson to Jameson, February 1, 1919, Jameson to Neilson, February 25, 1919, Patty W. Washington to Neilson, January 3, 1927, Neilson to Jameson, March 22, 1927, Dana C. Munro to Jameson, January 16, 1928, Jameson to Munro, January 18, 1928, James F. Willard to Munro, October 5, 1928, Neilson to Dexter Perkins, June 18, 1930, all in AHA Records. One of Neilson's undergraduate students, Dorothy Vastine, went on for the Ph.D. in medieval history at Bryn Mawr. Her adviser, William Roy Smith, credited Neilson with inspiring her with a love of medieval history and teaching her the necessary research skills. See Smith to Jameson, September 12, 1933, LC Archives.

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asked for their help. If they endorsed Neilson's candidacy, Williams urged them to write to other women and flood the nominating committee with letters of support.[75] Since Louise Phelps Kellogg was the only woman on the 1933 nominating committee, Williams wrote to her and urged other women historians to send their letters for Neilson to Kellogg. Williams not only assumed that Kellogg would support Neilson, she also believed that Kellogg commanded considerable influence among men in American history, since she had been elected president of the Mississippi Valley Historical Association in 1930. To Williams's surprise, however, Kellogg contended that she had "never been very much a feminist on these matters, feeling that work is the only criterion when it comes to honors."[76] Williams replied that she was "rather disturbed by what [Kellogg] seemed to think a feminist is," agreed that scholarship should be the sole criterion, and added that "we merely do not want women *discriminated* against because they are women." Surely, Williams believed, Kellogg knew of more than one AHA president who "has been far less of a scholar than N. N."[77]

Several women historians emphasized Neilson's scholarly credentials in letters to Kellogg, arguing that she truly deserved to be president. One historian from Minnesota maintained that she wished "to support the Feminist 'cause,' not blindly, but in such a case as this, when the woman in question is intrinsically capable."[78] As a young medievalist, Dorothy Louise Mackay contended that Neilson was an inspiration to her. She wrote Kellogg that her European colleagues "expressed some surprise that more recognition has not been publicly bestowed upon [Neilson] in her own country. They seem to think that Americans are unwilling to recognize merit in their women historians of such a reputation."[79] American male historians, however, were not about to be shamed into nominating Neilson.

Kellogg did write to her male colleagues in American history and to her fellow members of the nominating committee, who were unconvinced that Neilson was a worthy candidate. Although Kellogg encouraged women to write Neilson's name on the nominating ballots, she did not offer much hope that Neilson would appear on the final ballot.[80]

Realizing that it was imperative to muster support from leading men in the AHA, Mary Williams wrote to Conyers Read, hoping that he was "working too, especially with the *brethren*, who are more in need of salvation in this matter than the sisters, for the sisters are not likely to think that the sky will fall in if a woman presides over the A. H. A."[81] Read replied that he would work quietly to garner male support for Neilson but that a great deal of "anti-feminine bias" would have to be overcome: "Part of it is woman's own fault. She still wants the best of both

[75] Williams to Read, April 4, 1933, and April 5, 1933, both in AHA Records.
[76] Kellogg to Williams, April 18, 1933, Kellogg Papers.
[77] Williams to Kellogg, April 20, 1933, Kellogg Papers (emphasis in original).
[78] Marie Kohnova to Kellogg, April 20, 1933, Kellogg Papers.
[79] Dorothy Louise Mackay to Kellogg, April 21, 1933, Kellogg Papers.
[80] Kellogg to Mary Williams, April 18, 1933, Theodore Blegen to Kellogg, May 3, 1933, both in Kellogg Papers. Blegen wrote: "You ask for my reaction to the idea of making Nellie Neilson of Mount Holyoke V. P. of the A. H. A. The idea, as my son would say in his classic English, is not so hot." Also see Dumas Malone to Kellogg, November 15, 1933, Kellogg Papers.
[81] Williams to Conyers Read, April 4, 1933, AHA Records.

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worlds; the old world of chivalry and the new world of equality." Read added that he believed "if she is adroit enough she will get both."[82]

Williams also wrote to her colleagues in Latin American history, Clarence Haring of Harvard University, Stanford University professor Percy Alvin Martin, and the editor of the *Hispanic American Historical Review*, James A. Robertson. She confided to Robertson that many believed Neilson "is the most distinguished American medievalist, and that she should have been president of the Association before now. Unquestionably," Williams contended, "she is far abler than some who have sat in that seat of the historical mighty." If Robertson shared her view of Neilson, Williams urged him, "please work for her, especially among the men, for it is said that in order to get elected she will have to have strong masculine support."[83]

Williams wrote similar letters to other influential men, including William E. Dodd, then the first vice president of the AHA, and J. Franklin Jameson, who was a past president. While Jameson replied that he could not endorse a particular candidate, actually he opposed Neilson's candidacy for second vice president and instead favored Henry E. Bourne, then editor of the *AHR*. Bourne was also ten years older than Neilson, and age was considered in making the nomination.[84]

A campaign on Bourne's behalf, similar to the one waged by Williams on Neilson's behalf, was mounted by University of Chicago historian and editor of the *Journal of Modern History*, Bernadotte Schmitt. Schmitt urged his colleagues to flood the nominating committee with letters endorsing Bourne. Ultimately, like Williams, he was also unsuccessful, for Harvard medievalist Charles McIlwaine received the nomination and was elected as second vice president in 1933.[85]

Viewing Neilson's defeat as a temporary setback, women continued their campaign with renewed hope in 1934. That year, Kellogg became chair of the nominating committee and could if she wished, they believed, exert more influence on Neilson's behalf. Once again, women flooded Kellogg with letters of support for Neilson. On her own initiative, Kellogg solicited advice from other women. Viola F. Barnes, Neilson's colleague at Mount Holyoke, was ambivalent about her candidacy and urged Kellogg to be cautious in promoting her. While Barnes admired Neilson's scholarship and teaching ability, she was among those women who did not think Neilson was best suited to become the first woman president of the Association. Neilson, she argued, "has many of the qualities which men ascribe to women in general as reasons for not giving them positions of responsibility." Because of Neilson's lack of administrative skills, "equal treatment for women in historical scholarship would be set back by many years," Barnes believed, "if the first candidate in the important post of president should offer men critics the 'I told you so' opportunity."[86]

[82] Read to Williams, April 26, 1933, and April 5, 1933, both in AHA Records.

[83] Williams to Robertson, April 11, 1933, and May 9, 1933, both in Robertson Papers.

[84] See Williams to Dodd, April 18, 1933, Dodd Papers; Williams to Jameson, May 1, 1933, Jameson to Williams, May 4, 1933, both in JFJ Papers.

[85] Bernadotte Schmitt, form letter, May 27, 1933, Jameson to Schmitt, May 29, 1933, both in JFJ Papers; "Historical News: American Historical Association," *AHR*, 39 (1933–34): 593.

[86] Viola F. Barnes to Kellogg, April 21, 1934, Kellogg Papers. Neilson was the chairman of the History Department at Mount Holyoke and supervised Barnes' work; there had always been some friction between them. Thus Barnes' remark might be more a reflection on their relations than on Neilson's qualifications for the AHA presidency. See Glazer and Slater, *Unequal Colleagues*, 36–37.

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Kellogg apparently was not overly concerned by Barnes's reservations and promoted Neilson's candidacy among her male colleagues on the nominating committee. At the same time that women historians were agitating to have a woman elected to the presidency, a few liberal white male historians, among them Howard K. Beale of the University of Chicago, wrote to Kellogg to urge her to consider a black historian for nomination to the Executive Council or the nominating committee. He also noted, "The other matter is the recognition of some of our women historians. You have a position of importance this year. Miss Pierce, I believe, is on the program committee. But generally speaking, I think the women in our profession get far too little recognition in the association."[87] Kellogg then wrote to her fellow committee members to ask whether race and sex should be factors considered when choosing candidates to run for office. Not surprisingly, her male colleagues believed that blacks and women deserved no special consideration, and once again, Neilson was not nominated. Instead, the committee endorsed University of Minnesota historian Guy Stanton Ford, who was elected by the popular vote.[88]

Neilson did receive a consolation prize that year, however. In December 1934, in recognition of her scholarship, the Executive Council appointed her to the Editorial Board of the *AHR*. She was the first woman in forty years to serve on the board and was a member until 1940. With this appointment, her scholarly credentials were enhanced further, and her supporters could point to this service as an additional reason to elect her to the presidency.[89]

For the next several years, women campaigned unsuccessfully for Neilson. By 1938, they believed they had a golden opportunity to achieve their goal, for Vassar historian and Berkshire Conference member Violet Barbour was the chair of the nominating committee. When Barbour failed to convince her fellow committee members to place Neilson's name on the ballot rather than that of James Westfall Thompson for second vice president, she received petitions from more than fifty women historians, many from the Berkshire Conference. Although women waged a successful campaign and forced the nominating committee to place Neilson's name on the ballot with Thompson's, Neilson was defeated in the popular vote.[90]

Victory was within reach, however, for in 1939, Curtis P. Nettles and Howard

[87] Beale to Kellogg, May 12, 1934, E. Wilder Spaulding to Kellogg, May 14, 1934, Paul Lewinson to Kellogg, May 16, 1934, Kellogg, form letter to nominating committee, July 13, 1934, A. E. R. Boak to Kellogg, July 16, 1934, Richard A. Newhall to Kellogg, July 16, 1934, James Phiney Baxter to Kellogg, July 17, 1934, J. G. Randall to Kellogg, July 18, 1934, Kellogg, form letter to nominating committee, August 31, 1934, Kellogg to Bertha Putnam, September 6, 1934, all in AHA Records. Kellogg wrote to Putnam: "I have had many very pleasant letters about Miss Neilson and she was, I may tell you in confidence, the second choice of the committee for the Second Vice-President. I hope another year this may mature into first choice." Also see "Ballots for Second Vice-President," report and tally in Kellogg Papers, *ca.* 1934.

[88] See "Historical News: The American Historical Association," *AHR*, 40 (1934–35): 583.

[89] Minutes of the Executive Council Meeting, December 2, 1934, AHA Records; "Historical News: The American Historical Association," *AHR*, 40 (1934–35): 583. Neilson's position on the editorial board not only enhanced her own credentials, she was also able to assist more women in getting articles published in the *AHR*. Nine women published articles from 1935 to 1940.

[90] August C. Krey to Barbour, October 25, 1938, Barbour, form letter to nominating committee, December 19, 1938, Krey to Barbour, December 21, 1938, all in Krey Papers; "Historical News: The American Historical Association," *AHR*, 44 (1938–39): 767.

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FIGURE 8: Judith B. Williams, 1948. Courtesy of the Margaret Clapp Library, Wellesley College.

K. Beale, who were more liberal than most of their male colleagues, were elected to the nominating committee. Judith B. Williams of Wellesley College was the female member of the committee. Paul H. Buck was elected to the committee in 1940, and Beale became the chair. In May 1940, the committee sent a letter to all AHA members, reporting that "a considerable group of the Association's membership (men and women) feel that a small but notable group of women of distinction have not had sufficient recognition among the Association's officers."[91]

[91] Nominating Committee, form letter to AHA members, May 1, 1940, AHA Records; "Historical News: The American Historical Association," *AHR*, 45 (1939–40): 745.

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The committee urged AHA members to nominate women as well as men and to exercise their right to vote. Thanks to Beale, Buck, Nettles, and Williams, as well as women historians, Neilson was nominated and finally elected as second vice president in 1940.[92]

"WHAT THE FUTURE HOLDS FOR THE WOMAN who seeks the Doctor of Philosophy degree in history," Hesseltine and Kaplan wrote in 1943, "is as dark as the future always is."[93] While they hoped that the war would lead to increased employment opportunities and a greater proportion of women in the profession, the situation actually worsened. The proportion of women declined, and they continued to have difficulty securing employment; when they did, they were usually paid less than their male colleagues, regardless of institution, age, rank, or scholarly accomplishments. In 1930, 16 percent of academic women historians were full professors; in 1970, there were none. From World War II through the 1960s, recognition of women in the AHA also declined. Fewer women presented papers at annual meetings, fewer served on committees, and women were not elected to office in the same proportion as earlier years. The hope that Neilson's election would mean greater recognition for women in the future was dashed on the rocks of entrenched sexual discrimination. Forty-five years would pass before another woman would be elected president.[94]

Although female historians had by 1940 acquired an organization in the Berkshire Conference and a small measure of recognition from their male colleagues, these gains were fragile, part of what sociologist Jesse Bernard termed the "flowing and ebbing tides" of professional women's status. Female historians, like women in other professions, were increasingly marginalized in the 1940s and 1950s. Only in the 1960s did large numbers of women reenter the profession. Many turned to and developed women's history as a means of advancing the position of women. Women historians and women's history are now well integrated into the profession. Less certain is whether the increasing presence of women and women's history will produce full equality for female historians.[95]

[92] "Historical News: The American Historical Association," *AHR*, 45 (1939–40): 745. Also see "Historical News: The American Historical Association," *AHR*, 46 (1940–41): 748.

[93] Hesseltine and Kaplan, "Women Doctors of Philosophy in History," 257.

[94] "Appendix C," and Rose Report, 1970, AHA Records; Link, "American Historical Association," 5; Graham, "Expansion and Exclusion," 772; J. F. Wellemeyer, Jr., "Survey of United States Historians, 1952, and a Forecast," *AHR*, 61 (1955–56): 339–52. Wellemeyer points out that the actual numbers of women receiving Ph.D.s in history did not decline but that the ratio of female to male degree recipients did. Barbara Miller Solomon makes this same point for women doctorates in all fields. See *In the Company of Educated Women*, 191. On women's employment during the 1940s, see Susan M. Hartmann, *The Home Front and Beyond: American Women in the 1940s* (Boston, 1982); D'Ann Campbell, *Women at War with America: Private Lives in a Patriotic Era* (Cambridge, Mass., 1984).

[95] Jesse Bernard, *Academic Women* (University Park, Pa., 1964), Chapter 2, "Flowing and Ebbing Tides"; Jenson, "Committee on Women Historians," 8. Jenson suggests that women still lag behind their male colleagues in terms of salary, rank, and rate of employment and that the longer women remain in the profession, the greater the disparity. Also see Joan Wallach Scott, "Gender and the Profession of History: American Women Historians, 1884–1984," *Daedalus* (Fall 1987): 93–118, reprinted in *Gender and the Politics of History* (New York, 1989).

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August 1991 as Seen by a Moscow Historian, or the Fate of Medieval Studies in the Soviet Era

YURI L. BESSMERTNY

THE EVENTS OF AUGUST 19–22, 1991, IN RUSSIA, have been widely covered and analyzed in various mass media and by various authors. The outline of the events and their preconditions are known today not only to historians but to the broad public as well. I would like to offer a more specific characterization of these events. I want to view them in the light of the long-term processes embracing many decades of Soviet history and, at the same time, in the light of individual human lives, the lives of ordinary people, who are the first to be affected by any social or political change. It is not an easy task to combine these two aspects; to integrate them, I have chosen a peculiar genre, an analysis of my own life combined with an analysis of political events.

I was born and reared in Russia, and I hope to live in my country for the rest of my life, no matter how difficult or even tragic Russia's future might be. Before 1989, I had not visited Western countries, even though I had long been engaged in studying the history of the West European Middle Ages. The story of my reaction to the events of August 1991, the reaction of my friends and colleagues, our understanding of the impact of the coup on us and on the fate of the branch of history in which I have worked for over forty years, the Russian school of the West European Middle Ages, may shed some light on the coup itself and on its importance for today's history. In other words, I would like to approach the Moscow of August 1991 subjectively and also try to understand these events through a retrospective of the history of Soviet medieval studies.

I came to the study of history very late, after age thirty-five. The reason was not only that I, like all my countrymen of the same age, spent my years from age eighteen to twenty-two at the front of World War II. There was something more. After the war, at twenty-five, I graduated from Moscow University and was eager to continue my studies in the history of the European Middle Ages. The year was 1949, vacancies had opened up nearly everywhere, and I had fairly good recommendations. The obstacles I stumbled upon came from quite a different quarter. My professors told me that I should not even try to gain admission to postgraduate studies at my alma mater, since candidates there had already been appointed by the university party organization. But my attempts to enter another Moscow institution also ended in defeat: I was not admitted because the local authorities had beforehand decided to give preference to a young woman of

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whose loyalty and Marxist orthodoxy they were sure, whatever her scholarly or intellectual achievements might have been.

What was so special about me that I did not suit the authorities? No one told me anything definite officially. But my own experience, as well as the experience of my friends and acquaintances, allowed me to guess. I am Jewish. At the beginning of the postwar period, and even before that, Jews were more and more often subject to discrimination. In the first postrevolutionary years until as late as the mid-1930s, the Soviet authorities did not consider a Jewish origin a "presumption of guilt," or in fact, worth special attention at all. (The same attitude applied at that time to all other nationalities—or nearly so.) But the situation changed in the course of the repressions of 1937 and the following years. The new attitude reached a peak when the campaign of struggle against so-called cosmopolitanism was unleashed in 1949, just at the time of my graduation. Unlike the previous secret "campaigns," this one was waged openly in all the mass media of the country and was particularly sophisticated. The ideological subterfuge for the campaign was the statement that in the midst of the Soviet intelligentsia there had arisen a tendency "to kowtow to everything foreign," which could not be justified: extolling things foreign, people were apt to forget the achievements and merits of the Soviet people, and the tendency was therefore anti-patriotic and hostile by its very nature. The majority of those labeled "cosmopolites," 90 or 95 percent, consisted of people of Jewish origin, as could be discerned from their broadly publicized names. But nothing was said aloud, just as in my own situation. Among the Soviet Jews who, like me, were absolutely devoid of national consciousness and who were incapable of understanding the hidden meaning behind these events, hopes and even an optimistic belief that everything would be all right remained alive. Where this naïveté sprang from deserves a special analysis, as it was a feeling shared by many.[1]

LET US RECALL first that the "administration of the Soviet Union," which attempted to seize power on August 19 of last year, had as one of its principal slogans a return to the questionable notion of patriotism, "Soviet patriotism." The "Appeal to the Soviet People" published by the GKChP (State Committee for the State of Emergency)—the new organ of state power, formed during those days in August—called for "Soviet patriotism" without delay. Gennadii Ianaev, the man who usurped the post of Mikhail Gorbachev, called in his press conference for a restoration of the lost patriotic sentiment of the people. Some military leaders spoke even more openly. According to our post-coup press, General Albert Makashov in the coded communications dispatched to his troops invoked the notorious "cosmopolites" again, listing among them malicious propagandists who must be arrested on sight as the worst enemies of the GKChP.[2]

[1] This naïveté and conformity were typical, certainly, but not for all. (Compare Vitalii A. Rubin, *Dnevniki, pis'ma* [Jerusalem, 1988].) But among those around me, and especially those of my age, this feature was predominant.

[2] At the time of the putsch, General Makashov was commander of a military district in the Urals. According to the daily *Izvestiya*, no. 209 (September 3, 1991), Makashov, in a cipher message of

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This sentiment can be seen even more clearly in the "Appeal to the People" published a month before the coup by chauvinist and anti-Semitic organizations in Moscow. It is well known now that the "Appeal" was also signed by the future perpetrators of the coup, among them the then-commander-in-chief of the Soviet ground forces, General Vladimir Varennikov, the first deputy minister of the interior, Boris Gromov, the secretary of the Party of Russian Communists, G. Ziuganov, and others.[3] Moreover, there is reliable information that, on one of the first days after the coup, an editorial (prepared beforehand) was to be published in all newspapers with the title: "The Plot of Zionists and Masons Foiled by the Introduction of the State of Emergency." Of course, I had no way of knowing these things on the first day of the coup, but the general tendency was clear from the start. Therefore, for myself, my family, my friends, whether Jewish or not, for all the decent people of whatever nationality, the days of the coup were a time to say goodbye to normal life.

It was not just anti-Semitism that heightened our concern about August 1991. Here I must return to my attempt to gain entrance to postgraduate education forty years ago. For the university examination board, I was persona non grata not only because of my Jewish origin. My past was marked by another cause for suspicion. In the spring of 1938, my father, an insignificant white-collar worker in a Moscow office, was arrested on a charge of spreading anti-Soviet propaganda. After several days, he was sentenced to ten years imprisonment without the right to receive letters, and, as I came to know later, he died in less than six months at the notorious Kolyma gold fields called Lower Oturiakh, not far from the coast of the Arctic Ocean. My father's arrest had made a deep impression on me, then a boy of fourteen. It was not only the scene of the nighttime search and arrest, of hurried goodbyes to my father as he was led away. It may seem strange, but most traumatic was the fact that my father had worshiped, all but idolized, the Soviet system. He often reminded me, "But for the revolution, I would have stayed within the Jewish Pale of Settlement with no prospects at all." He reveled in every success of the country, be it the voyage of the icebreaker *Cheliuskin* along the Arctic coast of the Soviet Union, the records of our aviators, or the construction of new factories. And he did so irrespective of our own difficult life, full of hardships: my father's salary was hardly enough to make ends meet. Nor did the threat of purges hanging over my father's head dissuade him. After one such purge of Soviet white-collar workers in 1930, my father was sacked as a person of "non-proletarian origin." As a result, he could not find a job in Moscow, and for a year he had to live far from the family, several thousand kilometers away. What allowed him to remain enthusiastic about the Soviet system? Political naïveté? Ignorance? Unthinking acceptance of the then-popular idea of building a socialist paradise on earth? I do not know the answer to this question, and not only in my father's case but in that of many other people of his age as well.

August 20, 1991, ordered all the commanders of the regiments in his district "to strengthen patriotic work in military units . . . to arrest emissaries, cosmopolites, traitors of our Motherland and the Soviet Union, interrogate them and give them to Security once their identities are established."

[3] "The Appeal to the People" was published by a number of right-wing newspapers and magazines; see *Moskovskaia Pravda*, July 23, 1991.

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In 1938, other questions dominated my thoughts. I could not comprehend what was happening, why there was no justice, and why my father had to be sacrificed to the process of creating "a better life for all the people of the country." I am ashamed to confess that to me the most convincing explanation of what had happened to my father seemed to be a proverb horrible in its wickedness: "You cannot make an omelet without breaking eggs." I saw my poor father as one of these eggs doomed by the process of making the omelet. But that was only one of my thoughts. Another question inevitably arose: if my father, an entirely guiltless person, had not been able to prove his innocence, who could be sure of a safe tomorrow? Neither my mother, nor I, nor my friends. Disaster might befall us any day, at any moment. Soviet power was almighty, while we were absolutely helpless.

Since then, fear has governed me for a very long time, fear of the powers that be. It has brought with it timidity, deprived me of the inner freedom that should belong to man, and increased my diffidence. There were sufficient grounds for my feelings. Even before World War II, at one of the colleges I tried to enter, I was told that it was no place for the children of "enemies of the state." When I came back from the war front hopeful that I had atoned for my "sin," my experience in trying to gain admission to postgraduate studies dashed that hope. Later on, in Khrushchev's times, my father was posthumously "rehabilitated." But neither in those "new" times, nor later, up to 1989, was I permitted to take part in scholarly conferences abroad or even to travel outside the country, being a *nevyezdnoi*—bureaucratic slang for a person forbidden to go abroad. This label may have been earned by my own acts (of which I shall speak later). Yet the memory of the events of 1938 never left my mind as, in all probability, it had not left my file in the KGB.

In view of these experiences, you can imagine what kind of memories the coup aroused in my friends and me. The perpetrators made no secret of the type of "law and order" they were going to restore. On the morning of August 19, when I arrived at my institute, one of my younger colleagues, a very friendly person, asked me half-seriously, half-jokingly, "What? You are still at large? Not arrested yet?" Nobody doubted that massive arrests were to start any day, any moment. My friends and I believed that we were on the threshold of a return to 1937.

Later information confirmed our worst fears. After the failure of the coup, the victors found 300,000 arrest warrants for Moscow alone, filled out and signed, ready to be used. Factories in several towns had received orders to produce hundreds of thousands of handcuffs. Just before the coup, repairs had been started in some prisons and concentration camps. On August 19 and 20, we did not yet know these things, but our premonitions of disaster were not misplaced.

And why should my friends and I be arrested? What could have incriminated us in 1991? From a formal point of view, absolutely nothing. I had never belonged to a dissident group that acted openly before perestroika, had never publicly expressed anti-Soviet views; I did not even join any of the new parties that sprang up like mushrooms after 1988. My political inactivity may be attributed to reasons

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of character, one of which is no doubt the fear, whether conscious or unconscious, that has haunted me since my childhood. The more interesting question is why I and others like myself who were well-behaved citizens during the Gorbachev era looked on the coup as a threat to our lives.

I will try to answer this question by viewing it once again through my own experience. At the beginning of my scholarly career, like many historians in the 1950s, I was fond of agrarian history. Encouraged by my university tutors, A. Neusykhin and S. Skazkin (who had belonged to the prerevolutionary Russian school of agrarian history), I wrote and published several works on German agrarian history from the ninth to thirteenth centuries. These works, as I see them now, were fairly professional, though wholly in a Marxist vein. The "Khrushchev thaw" created conditions that allowed me to receive an appointment at a research institute of the Academy of Sciences of the USSR. Yet my scholarly conformity did not last long. Within a year or two, I was sick and tired of repeating the same formulas. More and more often, I worked on subjects unconventional among Soviet medievalists at that time. For example, I preferred social history to economic; I did not try to show "the inevitable and constant worsening of the peasants' living standards," but I attempted to see the real changes in the peasants' conditions, even if the change was for the better. The same occurred in regard to the topic of class struggle, a favorite theme of Marxist studies. I was more interested in the relations within a social class than between classes. I preferred to study the history of medieval chivalry rather than of the producers of material values.

I do not mean in the least that I denied the value of a Marxist approach to history. I was only trying to extricate myself a bit from those fetters and approach medieval history from a broader perspective. This appeared, however, to be quite enough to give me a reputation as an ideologically unreliable person. Fortunately, at the time of the "Khrushchev thaw," the institute authorities saw a need to make use of people who could offer something different from moldy old truisms. It was at this time that I managed to publish my first book,[4] a work in which I tried to prove that the West European medieval village had no social classes and to stress the noneconomic nature of most of the social relations. I did not have to wait long for the reaction of orthodox historians. A colleague of mine with whom I had been on friendly terms suddenly turned against me and leveled sharp criticisms. He included me in the group of well-known medievalists and historians of classical antiquity who sought to revise key problems in the history of pre-capitalist societies. We were all accused of "subversive ideological activities" directed against the basic principles of Marxism-Leninism.

The article in which he made these charges was not published in some obscure, insignificant journal: it appeared in the central organ of the Communist Party of the Soviet Union, called "The Communist."[5] Some people regarded this attack as a signal for a new round of witch hunts. Fortunately, that dismal prediction did

[4] Iu. Bessmertnyi, *Derevnia i rynok v Zapadnoi Evrope 12–13 vekov (po severo-frantsuzkim i zapadno-nemetskim materialam)* (Moscow, 1969).

[5] A. I. Danilov, "O metodologicheskikh iskaniiakh nekotorykh sovetskikh istorikov," *Kommunist*, no. 5 (1968).

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not come true, for repression, which had been easy to launch in the 1930s and 1940s, frequently misfired now that a new generation of people nicknamed "the men of the sixties" (*shestidesiatniki*) had come to the fore. But many professional medievalists and historians of antiquity approved of this criticism of us. It gave these lazy minds a chance to save the moldy ideas they had been promoting for years and to teach a lesson to the adherents of so-called scholarly ad-libbing (*otsebiatina*).

Thus the formation of two opposite trends in our medieval studies (and not only in medieval studies) began. These trends were given various names: one was called "Marxist," the other "structuralist" (at one time, my colleagues and I were accused of being too fond of structuralism). One was "orthodox," the other "revisionist" (to "revise" Marxism was at one time considered equal to being an "enemy of the people"). In recent years, the labels became more civil, like "official" and "unofficial" or "classical" and "non-classical." Yet the essence of the differences was always the same: the new trend in medieval studies, with its variety of themes and approaches, differed from the old in its refusal to act on the principle of pleasing authorities by following official ideology, in its denial of the axiomatic character of Marxist methodology, and in its decisive rejection of the traditional, a priori dismissals of non-Marxist historiography.

READERS SOON NOTICED THE DIFFERENCE between the two camps. The scholarly papers read by historians of the new trend (such as S. S. Averintsev, Iurii M. Lotman, Viacheslav V. Ivanov, Alexander P. Kazhdan, Leonid M. Batkin, Aron Ia. Gurevich, Mikhail Gasparov, Elena M. Shtaerman)[6] attracted large crowds of listeners; sometimes, young people tried to storm the lecture halls in which these scholars were to speak. Their publications were enormously successful, and the authors were invited everywhere, surprisingly enough even to address audiences of physicists, chemists, and biologists. All this happened in the years of purported "stagnation," evidently an indication of the growing number of people impatient for change, if only among the intelligentsia. Naturally, the reception enjoyed by such work could only make the ideological confrontation between the two camps more acute.

The traditionalists did not loiter. When I managed to publish an unorthodox article on forms of peasant dependence in Western Europe in the Early Middle Ages, their response was swift. Although the essay appeared not in Moscow but in a collection of articles published by the provincial university press of Nizhnii Novgorod[7] and might have seemed unlikely to cause any trouble whatever, some "anxious scholars" soon appeared in Nizhnii Novgorod, and Moscow as well, who were eager to attack an attempt to deny the serfdom-like (*krepostnicheskii*) character of peasant dependence in Western Europe. This would be tantamount to saying that West European peasants were different from Russian serfs and could therefore provide a point of departure for disparaging historical laws, for

[6] See below, notes 17–23.

[7] "K tipologii form krest'ianskoi zavisimosti v stranakh Zapadnoi i Iugo-Zapadnoi Evropy," in *Strany Sredizemnomoria v srednie veka*, vol. 2 (Gorkii, n.d.).

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allowing an "anti-patriotic slighting" of Russian feudalism, and for ignoring "the commandments" of V. I. Lenin. Next, the Academic Council at the Ministry of Higher Education was convened. About two dozen professors adopted a resolution condemning the article as "fallacious" and the editorial board as lacking in vigilance. The ministry had not yet even asked historians for their opinion on the matter when a number of scholars at Nizhnii Novgorod University raised an outcry, asking why the university staff's political reliability and fidelity to party principles should be doubted because of some medievalist from Moscow. The problem was quickly solved: the article was condemned, the publication of the collection stopped, and the medievalist in question suspended from teaching.

This was small trouble. Another example may be more compelling. A large group of researchers representing several institutes had for ten years, beginning from the late 1960s, been working on a vast project: a history of the West European peasantry in the Middle Ages. This work was the only research for which many scholars in the group received salaries, and for those times, rather large salaries. Several volumes were completed, a new general conception suggested, and some original conclusions made. It was these features of the project and the fact that medievalists of the "unofficial trend" took part in the collective work that determined its fate.

The minister of education himself joined in the discussion to approve or reject the collective work for publication. This man, Alexander I. Danilov, had once been a medievalist and a well-known one at that. Danilov easily persuaded the orthodox professors to kill the project. The method they used was absurdly simple and reeked of old times. Danilov stated that the project tended to revise the well-proved concepts of Marxist medieval studies, and, for the participants of the discussion and especially for the authorities of the institute, it was an argument not to be disputed. All the attempts of the chief editor, a much-respected academician,[8] to defend the work, as well as my own exhortations (I was then deputy-chief editor), were skillfully countered. A certain medievalist in our institute, a party functionary, called the chief editor aside and asked him, "Hasn't instructor So-and-so of the Central Party Committee spoken to you yet? Mind you, he has quite a negative opinion of this work." The same person went to the institute authorities and informed them of the same fact. Did the "negative opinion" of the highest authority on matters scholarly really exist? I never found out, but the essential thing is that a mere supposition that "above," among the powers that be, a "negative opinion" might exist, making the publication of a special work on medieval history undesirable, was sufficient to have the project dropped for the next eleven years. Only in 1985–1986, after the beginning of perestroika, was the work published.[9]

[8] While the project was being prepared, several chief editors replaced each other; the first was Sergei D. Skazkin, but at the approval stage for volume 1, the post was held by Lev V. Cherepnin.

[9] *Istoriia krest'ianstva v Evrope: Epokha feodalizma*, 3 vols. (Moscow, 1985–86). One of the hypotheses that is of importance in the history of the European peasantry concerns the causes of the origin of a dependent peasantry. We denied the concept of the dissolution of the peasant community as the principal cause of dependence; we denied the idea that inequality played the main part in the phenomenon. To put it simply, the dependent state of the medieval peasantry came as a natural consequence of the process of socialization of a barbarian society (I am speaking now of regions of

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How could it happen that dozens of important scholars, reviewers, members of scholarly councils, researchers at different institutes—the people who determine the fate of a publication—kept silent in the face of the arbitrary intervention of a few *apparatchiks*? It becomes comprehensible only within the context of the history of the Soviet intelligentsia. The first purge of Russian intellectuals took place in the early 1920s. In 1922, 160 disobedient members of the intelligentsia were deported from Russia by the decision of the GPU (State Political Department), without trial or hearing. Among them was the rector of the University of Petrograd, the well-known medievalist Lev Karsavin, one of the first researchers of medieval *mentalité*, the author of numerous books on the history of religious life in Italy during the twelfth and thirteenth centuries, on the philosophy of personality, and other themes.[10] This purge was a warning to other intellectuals, those still not willing to obey. Within a couple of years, Mikhail Bakhtin was arrested and exiled from Leningrad; his work on Rabelaisian France is widely known.[11] At about the same time, another outstanding specialist in the history of medieval culture and West European Christianity, Robert Vipper, was forced to emigrate from Russia.[12] At the end of the 1920s, academician Dmitrii Petrushevskii, the author of several remarkable books on English feudalism, became the helpless victim of disparaging criticism, and the Institute of History, of which he had been the director, was closed.[13] Petrushevskii's nearest disciple, one of the most talented students of the West European Early Middle Ages, Alexander Neusykhin, a person of rare human qualities and the highest moral standards, was dismissed and for a long time unable to get a job in his special field.[14] The

comparatively weak Romanization). The socialization subjected villagers either to public powers or to "private powers" of various forms.

Another interesting hypothesis in the work refers to the understanding of peasant culture. Unlike Bakhtin, we strove to speak not of two cultures, and not even of intertwined cultures. Together with Gurevich, we believe that medieval culture had a certain shared vision of the world, common to all social groups. This does not exclude the view that in different groups (social and intellectual as well), this shared vision could find different expression and could involve additional new ideas and attitudes.

A third hypothesis deserving attention concerns European servitude. It is sharply opposed to the notion of serfdom and to the later servitude in Eastern Europe during the sixteenth to eighteenth centuries. To my mind, the English villeinage or the German *Leibeigenschaft* are exceptional, not typical examples of European servitude.

The fourth hypothesis refers to the problem of choosing criteria for a typology of the development of the peasantry in different geographic regions and time periods. We stressed the importance of a system of criteria that includes not only the forms of peasant dependence but also the structure of the ruling class, the character of the peasant household, natural and climatic conditions, and other factors.

[10] L. P. Karsavin, *Ocherki religioznoi zhizni v Italii XII–XIII vekov* (St. Petersburg, 1912); *Osnovy srednevekovoi religioznosti, preimuchshestvenno v Italii* (Petrograd, 1915). See also L. P. Karsavin, *Dialogi* (Berlin, 1921).

[11] M. M. Bakhtin, *Rabelais and His World*, Helene Iswolsky, trans. (Cambridge, Mass., 1968).

[12] R. Iu. Vipper, *Vliianie Kal'vina i kal'vinizma na politicheskie ucheniia i dvizhenniia XVI veka: Tserkov' i gosudarstvo v Zheneve XVI veka v epokhu kal'vinizma* (Moscow, 1894); *Obshchestvennye ucheniia i istoricheskie teorii XVIII i XIX vv. v sviazi s obshchestvennym dvizheniem na Zapade* (Moscow, 1900).

[13] D. M. Petrushevskii, *Vosstanie Uota Tailera* (St. Petersburg, 1897–1901); *Ocherki iz istorii srednevekovogo obshchestva i gosudarstva* (Moscow, 1907); *Ocherki iz istorii angliiskogo gosudarstva i obshchestva v srednie veka* (St. Petersburg, 1903).

[14] A. I. Neusykhin, *Drevnie germantsy* (Moscow, 1929); *Die Entstehung der abhängigen Bauernschaft als*

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well-known Petersburg medievalists Nikolai Kareev and Ivan Grevs were deprived of practically every opportunity to publish their works, although they retained their professorial positions.

The situation grew even more oppressive in the 1930s. The continual fear of arrest and physical extinction locked tight the minds and souls of the people. An episode from the life of one of the brightest Moscow medievalists may serve as an illustration of the unbearable atmosphere of those years. Unable to live under the constant threat of arrest, this man went to the NKVD (the Peoples' Commissariat of the Interior) and asked the officers to arrest him, as he was not "Marxist." Though not arrested, he soon went mad. These were the conditions that caused an inner degeneration of the scholarly intelligentsia in the Soviet Union. Unfettered thought, the spirit of free inquiry, and attachment to world culture were disappearing from their moral code; spiritual timidity, subservience, and dogmatism took their place.

A sinister part was played in the suppression of free thought among historians by party decrees published in the first postwar years.[15] Nearly all independent medievalists, those who somehow managed to stand their ground, were accused of "kowtowing to Western culture" when they made use of the work of Western scholars, of an "objectivist" (that is, *not* abusive) attitude toward non-Marxist science, of overestimation of the role of ideas in history, and of so-called juridism (excessive attention to the terms and formulas of documents). These criticisms were encouraged by the campaign of anti-cosmopolitanism mentioned earlier.

Only during the years of the "Khrushchev thaw" and after the 20th Party Congress was there a short break in this type of criticism, and it was during that break that the new trend in Soviet historiography came to life. Unfortunately, it could not alter the overall situation.

FOR MANY DECADES, Soviet historians, medievalists among them, lived under the sword of Damocles. They were forced either to share all the views of their chiefs and so retain some possibility of professional activity or to reject them and gain a reputation as freethinkers, placing in jeopardy not only their professional activity and publications but their very physical existence. Understandably, even talented scholars usually preferred to conform.

The conditions that obtained before August 1991, and that allowed a restoration of the situation before perestroika in the field of history, continue to exist. One of the most important is the continuing influence of conservative scholars in the universities and other educational institutions. (One must keep in mind that our research institutes are almost completely divorced from the educational system of the country.) University students are taught today nearly the same things they were taught half a century ago. Very characteristic are the contents of

Klasse . . . in Westeuropa vom 6. bis 8. Jahrhundert (Berlin, 1961); *Sud'by svobodnogo krest'ianstva v Germanii v VIII–XII vv.* (Moscow, 1964).

[15] I mean the well-known decrees of the Central Committee of the CPSU of 1946, "On the Magazines *Zvezda* and *Leningrad*," which determined exactly what writers, critics, and all scholars of the humanities must and must not do.

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the recently published two-volume textbook of the history of the Middle Ages for history departments of universities.[16] It is the only textbook used in universities throughout the country for the study of medieval history, since it is practically impossible to publish competing works. The textbook has some merit: there are a few interesting chapters in it. But what about its general conception? The book retains many of the old dogmatic assertions that have been imposed on students in higher education for many decades. The editors still insist on the need to counter "bourgeois history" with Marxist methodology, as Marxism is able to reveal the whole truth, while bourgeois history (so the introduction claims) "has not been able to suggest any unified definition of the notion 'the Middle Ages'" (vol. 1, p. 6). Marxist historians, on the contrary, have been quite successful in creating such a definition. The interpretation of the editors of the textbook is considered to be beyond doubt and shared by all Soviet scholars without exception (vol. 1, pp. 38–39).

The authors of the textbook reproduce the traditional conception of the so-called social and economic formation as well as the idea of the succession of the five principal methods of production: primitive, slave-owning, feudal, capitalist, and socialist (vol. 1, pp. 7–11; vol. 2, pp. 20 and *passim*). We read that "feudal society, as well as any other class society, was characterized at every stage of its development by the constant everyday struggle of peasants against their feudal lords . . . Massive anti-feudal uprisings of peasants and commoners were an integral part of the early bourgeois revolutions, which played a decisive role in the overthrow of the feudal system" (vol. 1, pp. 11–12). With these a priori statements, the authors and editors leave aside much that has been achieved by historical scholarship in recent decades, such as a new understanding of the specific character of the West European Middle Ages as a whole and the character of the transitional period from the Middle Ages to modern times. The authors of a number of chapters base their argumentation, as did their predecessors fifty years before, on citations from Karl Marx and Friedrich Engels. I am not trying to deny anybody the right to appeal to Marx and Engels for methodological instruction: historians can choose the philosophic maxims or general hypotheses they deem most convincing. But one cannot accept theory without a critical eye.

It cannot but cause general amazement that the editors of this recently published work are still pondering the world historical process with the explicit supposition of the victory of socialism. The creators of the textbook have somehow neglected to look out the window and see what is happening in the world around them. The traditional, or—to be more exact—conservative, trend in Russian medieval studies still holds its position rather strongly, and, I am afraid, it will not be an easy task to overcome it, not in the near future, anyway.

In the beginning of my essay, I mentioned the people for whom the victorious coup would have meant inevitable intellectual death. I meant, first and foremost,

[16] *Istoriia srednikh vekov*, 2 vols. (Moscow, 1990–91), a textbook for university history departments edited by Z. V. Udal'tsova, S. P. Karpov, *et al.*

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those of my colleagues who for the last twenty or thirty years have been opposing the conservative trend in historiography as a whole, and in medieval studies in particular. In the course of the last five or six years, the confrontation has become especially acute. There have appeared new inter-institute seminars: for example, the seminar on historical memory, under the direction of Iurii N. Afanasiev; on historical psychology, headed by Aron Ia. Gurevich; on the history of everyday life, headed by Georgii S. Knabbe; on the history of European civilizations, headed by Alexander O. Chubarian and Mikhail M. Narinskii; and on historical demography, under my direction. In the philosophy department of Moscow University, a new program on the History of World Culture was added, headed by Viacheslav Vs. Ivanov.

Some of the scholars of this pleiad of historians have become well known outside their research institutes. Of those mentioned, S. A. Averintsev and V. Vs. Ivanov were elected to the Congress of People's Deputies of the USSR; L. M. Batkin is now a leading publicist; Aron Gurevich is well known by his publications and public speeches in defense of free research in the humanities. (Even more popular now is Iurii Afanasiev, rector of the Humanities University of Russia, one of the leaders of the democratic movement.) All this is hardly accidental, as one of the most important, central aspects of the revolution in my country is the revival, I could even say resurrection, of the individual and individual freedom. It is the history of people (rather than of productive forces) that comes to the foreground in the scholarly works of most of our medievalists and historians of other specialties, those who since the 1960s have formed the new trend in Soviet historiography. It is only natural that their work should be closely connected with certain beliefs and attempts to defend civic ideals in everyday political practice.

Among the brightest scholars of this trend I would mention first of all Aron Gurevich. His works have been translated into English and some other languages (see in particular *Categories of Medieval Culture* and *Medieval Popular Culture*).[17] One of his principal ideas is the necessity to make use of the hidden dialogue between the pastor and his flock to comprehend stereotypical behaviors of the "silent majority" in medieval communities. Gurevich holds that such a dialogue may be "overheard" in virtually all medieval didactic texts. An analysis of such a dialogue should constitute the starting point for a characterization of common representations and attitudes, of common ideas of the world, society, and themselves, typical of the people of the Middle Ages. Even though most of these ideas were not held at a conscious level, Gurevich's approach has opened up new possibilities of understanding the stereotypical attitudes that must have guided the behavior of medieval people whether they were conscious of it or not.

Gurevich believes that comprehension of these stereotypes allows us to understand the "mechanism" of social existence, for all the events and processes in the world are directly brought about as a result of the perceptions of those who live and act in it. Thus Gurevich's approach to the study of mental structures may

[17] A. Ia. Gurevich, *Categories of Medieval Culture* (London, 1985); *Medieval Popular Culture: Problems of Belief and Perception* (Cambridge, Mass., 1988); see also *Srednevekovyi mir: Kul'tura bezmolvstvuiushchego bol'shinstva* (Moscow, 1990). At present, Gurevich is head of the project "Man in History" at the Institute of General History in Moscow.

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serve as a cornerstone for our understanding of the interrelation of events and processes on the one hand and common socio-cultural concepts on the other. In other words, this approach offers a new method of historical synthesis.

One of the most characteristic features of Gurevich's works, as well as of the new trend in Russian medieval studies as a whole, is a constant endeavor to evaluate epistemologically the means and methods of research. To what extent are the historian's conclusions subjective, when he or she makes use of different methods of research? How do these methods reflect the relativity of our knowledge of the past? These questions and their accompanying discussions take up a considerable part of the works of historians belonging to this trend and, in particular, the works of Leonid Batkin, author of a number of books on the history of the Italian Renaissance, among them a recent book on Leonardo da Vinci (Batkin's works have been published in Italy, Germany, and other countries). Batkin supposes that, for a greater knowledge of the past, it is important to keep to the "complementary principle," which implies that any research of mass performance, by the method suggested, for example, by Gurevich, should be complemented by research into the unique in culture and history. In other words, a sociological insight into a text must necessarily be followed by its "culturological" analysis. The latter, as Batkin states, supposes a certain "unique insight" into each individual text, which in its turn should be considered a unique sample of the intellectual image of a unique person of each given epoch. The combination of both methods results in a dialectic between individual freedom and the submission of an individual to mass stereotypes of behavior.[18]

Another aspect of epistemological anxiety of the historians of the new trend is the influence of "non-source knowledge" on the researcher. I mean the knowledge, ideas, and attitudes that have been formed willy-nilly in the mind of each historian before he or she starts an analysis of a text. First of all, this is the knowledge acquired under the influence of the works written by our predecessors; second, that formed under the influence of scholarly or philosophic theories of the historian's own time and place; third, the ideas, perceptions, and attitudes imposed by the interests and demands of the eventual reader of the historian's works. (As far as I can judge, no one can avoid possessing this non-source knowledge.)

This knowledge leaves its imprint on every aspect of the historian's research, but in some cases the imprint is more telling. In the demographic history of medieval France, I tried to show that in order to comprehend population development more objectively, two main principles should be kept in mind. We must constantly verify our understanding of the processes and events in the demographic sphere in the light of the concepts and perception of these processes and events by their contemporaries. Also, we must be aware that our non-source knowledge may distort our hypotheses about the processes and events of the past even more than our hypotheses about the stereotypical perception of these

[18] See Leonid M. Batkin; "Dva sposoba izuchat' istoriiu kultury," *Voprosy filosofii*, no. 12 (1986); *Leonardo da Vinci i osobennosti renessanskogo tvorcheskogo myshleniyia* (Moscow, 1990), 372-73 in particular. See also Leonid M. Batkin, *Die historische Gesamtheit der italienischen Renaissance* (Dresden, 1979); *Leonardo da Vinci* (Bari, 1989); *Gli umanistici italiani: Stile di vita e di pensiero* (Bari, 1990).

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processes by medieval people. The phenomenon of social processes is complex in its inner structure, and therefore it resists adequate comprehension. These were the principles I tried to illustrate in my book *Life and Death in the Middle Ages* just published in Moscow.[19] In regard to the new epistemological approach in medieval and ancient studies, I should also mention the works of Iurii Lotman,[20] a specialist in history, literature, and semiotics; Viacheslav Ivanov,[21] a linguist, philosopher, and Asian historian; Sergei Averintsev,[22] a philosopher and historian of literature. I want to stress, too, that these people have always tied their defense of their scholarly principles to a defense of the ideals of freedom and civil rights.[23] It is only natural that these scholars oppose any attempt at restoration of the old regime and that they receive the vigorous support of their students and numerous admirers.

I WOULD BE HAPPY TO CONCLUDE my essay on an optimistic note by saying that the future of Russian medieval studies, as well as the future of the whole of historical scholarship in my country, belongs to people like those I have mentioned. Alas, at present, there is no reason to be optimistic in this respect. The struggle is very far from over. The future of our medieval studies, like the future of many other spheres of intellectual life in my country, essentially depends on the outcome of the fight between those supporting the new tendencies and their adversaries. The confrontation goes far beyond the natural and customary limits of debate characteristic of the scholarly milieu elsewhere. In my country, as I have tried to show, these differences are expressed with intolerance. The struggle of ideas in today's Russian historiography is tightly interwoven with the struggle of political sympathies and antipathies. It is no less important that, during the recent seventy-odd years, tolerance toward dissident opinion and free thought in general was rooted out from every area of Soviet life. Our mental stereotypes, whether we

[19] Recently, a new occasional publication has appeared in Moscow, published by A. Gurevich as chief editor and myself as deputy-chief editor. The publication bears the name *Odysseus: Man in History* and prints papers by historians, philosophers, and culturologists of the new trend in our historiography. Some of the materials published in *Odysseus* have been reprinted in *L'Homme et l'histoire* (Moscow, 1990). Iu. L. Bessmertnyi, *Zhizn' i smert' v srednie veka: Ocherki demograficheskoi istorii Frantsii* (Moscow, 1991).

[20] Iu. M. Lotman, *Universe of the Mind: A Semiotic Theory of Culture* (London, 1990); *Semiotica e cultura* (Milan, 1975).

[21] V. V. Ivanov, *Einführung in allgemeine Probleme der Semiotik* (Tübingen, 1985). One of the most important works of Ivanov (in co-authorship with T. V. Gamkrelidze), *Indoevropeiskii iazyk i indoevropeitsy* (Tbilisi, 1984), has not been translated into foreign languages, as far as I know.

[22] S. S. Averintsev, *Plutarkh i antichnaia biografiia* (Moscow, 1973); *Poetika rannevizantiiskoi literatury* (Moscow, 1977).

[23] Here I would like to mention another name I have mentioned before briefly, a scholar of considerable importance: A. P. Kazhdan. Even before his forced emigration, he had played an outstanding role in medieval studies and published several books on the history of Byzantium and early Christianity. The Western reader may know such works of Kazhdan as *People and Power in Byzantium* (together with Giles Constable) (Washington, D.C., 1982); *Change in Byzantine Culture in the Eleventh and Twelfth Centuries* (Berkeley, Calif., 1985); *The Oxford Dictionary of Byzantium* (New York, 1986, 1991). See also Kazhdan's articles on Soviet medieval studies: "Soviet Studies on Medieval Western Europe: A Brief Survey," *Speculum*, 57 (1982): 1–19; "Livres soviétiques récents sur la culture du Moyen Age et de la Renaissance: Observations d'un byzantiniste," *Annales: E.S.C.*, 2 (1980).

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are conscious of the fact or not, are burdened with the quest for ideological dictates. The situation is further complicated by moral irresponsibility on the part of some scholars and the domination for several decades in both politics and ideology of the principle "the end justifies the means." Though never openly admitted, the principle was encouraged by political practice. As a result, even now, numerous enthusiastic adherents of the past are prepared to turn scholarly arguments into ideological struggles and to use any means to crush an opponent. Last but not least, the country's political instability aggravates the climate of mutual suspicion and disloyalty.

I am afraid that I can offer only these not very comforting observations as a conclusion to a medievalist's meditations on the coup of August 1991.

September 1991

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Reviews of Books

GENERAL

FRANCIS FUKUYAMA. *The End of History and the Last Man*. New York: Free Press. 1992. Pp. xxiii, 418. $24.95.

Francis Fukuyama would not be at the RAND Corporation if he were an avowed postmodernist. But the implications of his thoughtful essay point in just that direction. Like many cultural theorists today, Fukuyama argues that there are no longer any overarching plots or designs that give prescribed meaning to our political endeavors. History with a capital "H" has come to an end, although the lower-case history of births and deaths and private aspirations persists. This is the case because the twentieth century has knocked out all the ideological challengers to the principles of the liberal democratic order. "The last man" left standing is a democrat and a capitalist. And once there is a winner, there is no longer a contest. This Last Man has concluded History.

In similar fashion, postmodern critics in the 1990s have taken pains to distinguish themselves from their modernist counterparts of the 1890s, who still sought some order in the grand narratives and unified subjects of History. That Marxism, with its storyline of class struggle and its well-known proletarian subject, has crumbled as an intellectual tool in the academy is a striking indication of the present-day postmodernist sensibility. Both Fukuyama and postmodernists agree that the recent past is different from the present because yesterday was experienced as a dramatic contest of ideologies in a way that today is not. The only difference is that Fukuyama believes the contest to have been won while postmodernists argue that it was all theater to begin with. In either case, today, tomorrow, and the day after will simply proceed without a new or believable narrative in place to give events any coherence, a predicament of drift that is both novel and terrifying and poses once again Nietzsche's questions about the diminution of the human spirit.

Moreover, Fukuyama implicitly questions the further usefulness of historians since any meaning invested in the reading and writing of history depends in large part on the validity of grand narratives, partisan purposes, and good causes. Without these, history books become aimless, antiquarian, merely interesting. The end of History is thus the end of a genre, although not the foreclosure of events themselves.

The end of History rests on the victory of liberal democracy and capitalist economics. According to Fukuyama, no other competing system of thought has survived the twentieth century. The key dates denoting liberalism's triumph are 1945, the year the Allies defeated Nazism, and 1989, the year that marked the collapse of communism. Whatever the misgivings about democracy or capitalism, misgivings that Fukuyama is willing to concede, no one today imagines a radically better future that is not essentially democratic and capitalist. To be sure, the delights of the liberal order have yet to reach everybody, a process that might take several generations. But even if they do not, the incompleteness of liberalism is not necessarily its invalidation. According to Fukuyama, who in turn leans on Hegel and Alexandre Kojève, history only moves forward when the contradictions of one system give way to basic alternatives.

All the crushing poverty and bruising political tyranny around the world does not add up to an alternative order. If the rest of time is simply testimony to the incompleteness or slow implementation of liberalism's promises, it would still be the case, for Fukuyama, that History has ended. This is a point most of Fukuyama's critics have misunderstood. The idea of the end of History is not an argument for the rightness of all things. Fukuyama is not replaying Pangloss, even if he is too strident in his applause for Western politics and Western economics. Rather, Fukuyama's news is that we have stopped thinking about what might lie beyond capitalism and democracy. All that goes on now is tinkering with a system that variations such as Swedish-style social democracy and Reaganomics leave basically intact. In a fundamental way, Fukuyama asserts that we have all become neoconservatives; even leftists are mostly lapsed Marxists who are now simply social democrats. This disenchantment might be sad, but it rings true.

There are powerful reasons for the triumph of the liberal order. Fukuyama argues that 400 years of military and mercantile competition required states in Europe and elsewhere to adhere to rational and

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scientific principles. If this revamping of structural-functionalism and modernization theory explains the victory of capitalism, it is not sufficient to explain the ultimate triumph of democracy, which Fukuyama argues is the endpoint of humanity's species-unique struggle for recognition or *thymos*.

Fukuyama insists that the origins of democracy are to be found in the willingness of men and women to risk comfort and security in order to attain liberty and dignity, a point he makes by profitably comparing Hobbes with Hegel. Both philosophers agreed that people often act to gain prestige as individuals. These irrational endeavors lead to unfortunate episodes of imperialism and domination. It is precisely this self-proclaimed ethic of aristocratic mastery that Hobbes saw as the barrier to well-ordered government. Once citizens agree to live rationally by submitting to the laws of the monarch, they avoid the horrors to which irrational vanity leads. For Hegel, the Hobbesian submission to the state is little more than slavery, and vanity's contempt for this "mere life" is the beginning of freedom. The conditions of liberty are secured only once the slaves conceive in universal and this-worldly terms the freedom that nobles had once acquired one-by-one. History ends with the final revolt of the slaves, which Hegel identified as the French Revolution.

If all this sounds familiar it is because Fukuyama overstates the originality of his argument and, in fact, closely follows "the march of progress" that is still embedded in many modern history courses. Remember the class? German and Russian misdevelopment accents the righteous path of France, Britain, and the United States. For all the wrenching pessimism about the authenticity of progress that Fukuyama laments in post-Holocaust thought, the story of the twentieth century is told from what Michael Geyer has termed a North Atlantic or NATO perspective that invariably leads to a happy ending. It was in this North Atlantic realm that weak feudal traditions, more languorous industrialization, and strong civic traditions supposedly favored the advance of political democracy and measured social reform. By contrast, Germany's rise to nationhood after 1870 was turbulent in the extreme and turn-of-the-century Russia faced even more difficult social and economic circumstances. The price paid for the disfavors of history was Nazism in Germany and Bolshevism in Russia.

The story of the progress of liberal democracy depends on declaring fascism and communism exceptional and thus illegitimate challengers. Fukuyama, for example, argues that fascism was an aberration generated by "hothouse" industrialization and post–World War I chaos. A similar compound of bad luck left Russia vulnerable to communism. What this line of argument ignores, however, is the degree to which fascism and Marxism emerged out of the intellectual mainstream in Western Europe, and therefore they cannot so easily be separated from democratic thought.

It was the coldness and shallowness of the very rationalist forms of capitalism and bureaucracy that Fukuyama sees triumphant at the end of the twentieth century that spurred the search for new political communities at the end of the nineteenth century. Solidarities based on class, blood, nation, or generation seemed to offer more social intimacy and more spiritual purpose than did free-market liberalism. Political modernism after the 1890s was the composition of this emotional foundation to public action. It did not just prosper in those nations (such as Germany) that were scarred by rapid industrialization and the remnants of the feudal order. A glance at France at the turn-of-the-century moment of the Dreyfus Affair, for example, provides plenty of evidence for the widespread appeal of integral nationalism, strident racism, and exclusively working-class syndicalism.

The point is not only that racists or fascists or Marxists have much deeper roots in nineteenth-century thought than Fukuyama allows, but that they shared with liberalism basic assumptions about progress as well. It would be nice to think of fascists, for example, as improbable types, resentful antimodernists and the executors of history in its most extreme conditions. But their appeal was surprisingly broad and often rested on democratic or populist appeals, and their programs were technocratic and forward looking. Indeed, it is striking to see how unquestioningly most political groups in this century have invested in the vocabulary of modern science, diagnosing sickness in the population, deploying interventionist therapies to restore "health," classifying and sorting out physiognomic types, and doing so in the name of progress. The eugenic imagination is an excellent example of the commonalities among liberals, fascists, and communists. This common ground should give pause to those such as Fukuyama who dismiss any necessary link between modernism and the Holocaust. By the same token, total war in 1914–18, with its awesome mobilization of bodies and hearts and minds, was not an irksome German or Russian invention but the consequence of the democratization of war, a process that implicated each belligerent. Fukuyama stumbles across all sorts of history when he tries to separate the righteous aspects of the democracies from the sinful ones of the dictatorships.

It is precisely because the twentieth-century order has witnessed the menace of Nazis and Stalinists and also the menacing effects of technological protocols, scientific classifications, and popular nationalisms that thoughtful observers have become skeptical about progress. It is simplistic to accuse Western philosophers of being unduly pessimistic in the face of the Holocaust or Hiroshima. What leaves observers "speechless," as Fukuyama somewhat crudely puts it, is not the existence of evil as revealed by the Holocaust but overconfidence in the ability of science and rationalism to remake the social world, an arrogance

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on which no one side has a monopoly. The recognition of the fragile and destructive aspects to civilization is not out of place.

Fukuyama's collapse of modernism into well-ordered democracy has the unfortunate effect of sanitizing Western history. Fukuyama distinguishes a liberal telos from illiberal pathologies with unjustified rigor. At a certain point the menaces cannot be explained away as either exogenous aberrations or mere growing pains that do not impinge on the underlying principles—otherwise the ideal adds up to little more than ideology. Indeed, Fukuyama's distinction between democracy and totalitarianism rests on a total misreading of Kojève, the French philosopher and interpreter of Hegel on whom Fukuyama relies. For Kojève, the historical function of Stalin and Mao was to introduce the Napoleonic code to Russia and China, the severity of their methods being a function of tardy deployment rather than essential illiberalism, a view whose ironic statement and political implications a more attentive Fukuyama, for one, would certainly abhor.

None of this really compromises Fukuyama's central point: there are no robust ideological alternatives to liberalism, which enjoys broader worldwide acceptance than at any time this century. And Fukuyama is right in insisting that those constituencies who already enjoy the fruits of capitalist economics and liberal democracy do not intend to roll back this "progress." Modern history has taught us many lessons, Fukuyama maintains, with the result that we appreciate the modest successes of the present and remember the disastrous results of hubris in the past. This collective memory guards against history repeating itself. But if we remember, what do we forget? What are the irretrievable losses that have been incurred in the twentieth century? Another way of putting this question is to consider progress not simply from the point of view of the living but also of the dead.

I suspect that the nineteenth century would have been appalled at the twentieth century. Most nineteenth-century intellectuals did not endorse democracy as Fukuyama suggests. On the contrary, democracy was widely regarded as the vulgar rule of averages, a despotism of the masses to which fascism and communism attested. This perspective is rather different than Fukuyama's. Nineteenth-century thinkers also assumed that men and women would make use of new ideas and new tools in an effort to become more able and more worthy of their god. They could not easily have imagined a culture of passive spectators and consumers. If the twentieth century is happy not to have to live in the nineteenth, which was without anaesthesia, ice cubes, or indoor plumbing, the nineteenth century, in turn, must regard the twentieth as a nightmare of complacency. Fukuyama completely lacks the irony of Kojève, who acknowledged that the End of History is not accompanied by the fullness of wisdom. Perhaps progress exists only as the conviction of each present, plausible because the dead do not speak, credible because we have forgotten more than we have remembered. Is this not the cunning of History?

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HAROLD DORN. *The Geography of Science*. Baltimore: Johns Hopkins University Press. 1991. Pp. xx, 219. $35.95.

This is an occasionally intriguing and insightful but often ill-supported attempt to call attention to the environmental forces shaping scientific activity. Starting from a refined version of Karl Wittfogel's theory regarding the emergence and character of "Oriental Despotism," Harold Dorn argues that wherever "hydraulic agriculture" comes into existence, powerful centralized state structures emerge with the authority to conscript labor and manage the hydraulic works, and these powerful centralized structures spawn scientific "institutions" that create scientific knowledge aimed solely at serving the needs of the state. Practical sciences, including calendrical astronomy, arithmetical (rather than geometrical) mathematics, and agronomy, and divinatory sciences like astrology characterize all such hydraulic societies.

In his attempt to demonstrate the generality of this claim, Dorn explores not only the ancient civilizations of Mesopotamia and Egypt but also the early cultures of the dry northeastern portion of Sri Lanka, Mayan culture (where too much rather than too little water was the hydraulic problem), and the Pueblo cultures of the American Southwest. In one of his most fascinating and extensive examples, he details the response of the Latter-day Saints to their desert environment in the Great Salt Lake basin, showing that all of the typical features of hydraulic civilizations—including the creation of science-oriented higher educational institutions and an astoundingly high level of scientific achievement in practical sciences, such as agronomy and soil sciences—characterize the Mormon culture in Utah.

In contrast to the useful and communally supported scientific traditions associated with hydraulic civilizations, a "useless" (p. 74) and individualistic scientific tradition emerged just once in Hellenic Greece, where the dominant forms of agriculture depended directly on rainfall rather than on irrigation. Greek "rational" science was predominantly a kind of spontaneous response to individual curiosity about the natural world and was completely without any institutional support.

Among the most interesting of Dorn's arguments are those about science in cultures that have been the inheritors of both Hellenic and hydraulic traditions. In Islam, which had a hydraulic agricultural base, applied sciences dominated, while in medieval Eu-

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rope, whose agriculture was based on rainfall, the Hellenic pattern was most pervasive. But when a revolution in military technology created the early modern centralized nation-state, a new tension arose between the scientific elite, who perpetuated the Hellenic vision of science, and the state patrons of science, who perpetuated the hydraulic vision. This tension, Dorn argues, persists today: "In the value we place on pure research, we are Greek. In our institutional patronage of science, we are Babylonian" (p. 173).

Unfortunately, in support of his potentially interesting argument, Dorn uses terms in an idiosyncratic and irritating way, depends on dubious evidence, and indulges in eccentric and sometimes inflammatory editorializing. He insists, for example, that, "Plato's Academy and Aristotle's Lyceum were not *institutions*, since they had no commitment to public service" (p. 184, n. 11). Similarly, he later insists that the Royal Society of London was a "club" rather than an "institution" because it did not serve the interests of the public (p. 142). Such usages, while consistent, raise innumerable problems of justification. To support claims regarding the relative usefulness of science in different cultural contexts he creates a Robert Merton–like utility index based on his subjective impression of whether the *Dictionary of Scientific Biography* biographies of scientists drawn from those cultures seem to him to indicate whether the relevant figures were "pure" or "applied" scientists. Totally aside from the unaddressed problem of whether the sample scientists adequately characterize their different communities, this exercise becomes ludicrous when figures like Thales, Anaximander, and Archimedes are simply labeled "P" for pure without qualification. Finally, when Dorn claims that the "Bureau of Reclamation has commanded more power and exercised more authority than even a dynasty of Oriental despots" (p. 156), again without explaining the contextual differences, I simply see red.

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RICHARD OLSON. *Science Deified and Science Defied: The Historical Significance of Science in Western Culture.* Volume 2, *From the Early Modern Age through the Early Romantic Era, ca. 1640 to ca. 1820.* Berkeley and Los Angeles: University of California Press. 1990. Pp. xi, 445. $45.00.

In the early postwar era James Conant at Harvard and Herbert Butterfield in Cambridge both advocated that the history of science be taught as part of the general history curriculum. They did so partly in response to the challenge that nuclear science and technology presented to democratically constituted societies, and partly because they wished to document and illustrate science as one of the central achievements of Western culture. They sought to make rational that which had come to appear quite suddenly, as a result of World War II and then the Cold War, dangerously irrational. Not without irony when we look back at it, Thomas Kuhn said in *The Structure of Scientific Revolutions* (1962) that he had shown the book manuscript to only one other person, Conant. Kuhn strongly implied that he was attempting to fulfill Conant's mandate.

Since the 1960s all the worst fears of Conant and Butterfield might seem to have been realized. Instead of becoming part of the general curriculum, the history of science slipped quietly into the pattern of professionalization common to the academic disciplines. It became more specialized, less accessible, institutionalized in many cases in separate departments, and increasingly vulnerable to marginalization and, in hard economic times, to institutional extinction. At the same time historians of science, in revolt against definitions of the rational and the irrational, waged a roughly ten-year war between the internalists and the externalists, which most other historians did not, or could not, follow. By the 1990s the history of Western science does not even possess that most trusted of pedagogical companions, a textbook.

Richard Olson's book attempts to remedy that deficiency. It is clearly written, the science it presents is readable, the diagrams are clear, the jargon is missing, and the effects of the ten-year war are intelligently assimilated and addressed: science has a history that is profoundly related to the world that gave rise to it. In addition, the old cleavage between science and technology (the grunt used to be, "well, what did the *Principia* have to do with the Industrial Revolution, anyway?") is elegantly dismissed. By far the largest section of this volume is devoted to science, broadly conceived, and the Enlightenment. It could be used in any general course on eighteenth-century Europe. Likewise, economic historians could profitably assign the chapters on science and technology in the Industrial Revolution.

Olson's book partially succeeds in filling a hole as empty as Newton's vacuum. Qualifications must, however, be registered. For the seventeenth century, secondary works published after 1970 are seldom invoked. Of the many social analyses and critiques of the scientific revolution now available, only one not very good and very early feminist account is presented. The method of the book relies on a now quite dated kind of history of ideas that in this instance is only exaggerated by the author's tendency toward wordiness. A firmer editorial hand would have improved a book that must take its place as the only reasonably good textbook for the whole of early modern Western science since the 1960s. Perhaps it is the fate of any textbook that specialists will quibble and fuss with it. Before doing too much of that, we could also try writing one. Olson's book has its distinct limitations, but it should find a place in introductory

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courses from Western Civilization to surveys of British and French history.

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LUDMILLA JORDANOVA. *Sexual Visions: Images of Gender in Science and Medicine between the Eighteenth and Twentieth Centuries*. (Science and Literature.) Madison: University of Wisconsin Press. 1989. Pp. xiii, 207. $22.50.

Like the rest of the academy, the history of science and medicine is now engaged in cultural studies. How could fields that subject nature and bodies to their scrutiny avoid the gaze of Michel Foucault or the feminist attack on our phallocentric culture? But the scrutiny of "objective" science or professional medicine as cultural "mediators" is not easy. This book by Ludmilla Jordanova shows that in fact it can lead to enormous frustration and confusion. But the trip is worthwhile.

Jordanova has argued before that science is our cultural mediator. In particular she has identified the peculiar intensity of our search for knowledge about the earth's body and our own as important historical problems. Now she exhorts historians to become aware of the gender markings of this intellectual history with essays on the "images of gender in science and medicine between the eighteenth and twentieth centuries."

Because she portrays gender as a protean marker of our scientific culture, this book is frustrating to read as an argument. Many themes and assertions are presented in each essay; they are interrelated but do not cohere. The way into her story is through the notion of "woman as other." She writes about the binary male/female as constructed in our culture around the need of "groups and individuals [to] distance themselves from each other, often by unconscious means. Such separating devices are only needed, however, when the two parties involved are also deeply bound together, implicated in each other's characteristics" (p. 14). This book is thus part of the feminist, postmodern critique of categories and binaries of male and female, yet one that wants to explain how these categories and representations have had such power over our thinking, our "sexual visions."

The book also assumes a Foucauldian perspective on sex as "incited" by science and medicine in our modern culture. Unlike Foucault, however, Jordanova eschews the major institutions or dogmas of science for a study of a multitude of odd places, familiar and unfamiliar, where images of science, medicine and/or nature have been given a deeply sexual (and often deeply disturbing) cast.

The images in the book of medical men dissecting their female "anatomies" with eyes and scalpel could be telling us about the rapacious, aggressive sexuality of our culture. She opens with a parable from Ivan Turgenev's *Fathers and Sons* (1861) in which the doctor, "a cold man of science is confronted by [the] feminine warmth [of his fiancée]" and responds "'What a magnificent body!' Barazov pursued. 'Shouldn't I like to see it on the dissecting-table!'" We recoil in horror but Jordanova asks us to see beyond the aggression to the "social and cultural preconditions of such a statement" (p. 1).

Jordanova's first chapter, on "Natural Facts," establishes that it is not aggression but an addiction to simultaneous stability and instability that has sustained this odd quest for knowledge. She concludes that by the eighteenth century "a particular scientific style of studying human beings sustained the relationships between nature, culture and gender . . . characterized by an interest in people as integrated units . . . in relation to the medical domain in particular" (p. 37). By the nineteenth century this peculiar binary style was solidified into the hierarchies of biology and the social sciences, but it was never resolved because, Jordanova implies, our culture needed gender markers that were inherently contradictory.

Her book pursues the idea of the quest for a dissecting and yet holistic knowledge that can never satisfy, and that only raises more questions. In contrasting the shocking and yet subtle workings of this medical culture, Jordanova introduces the term "body image" to play off the particular body forms used by medical anatomists at the turn of the nineteenth century—the wax figures of pregnant and erotic females—with the influence of gender on the modernizing European mind. She realizes that this is quite a leap, made harder with her presentist social science terms and with her omission of a social analysis of those who gazed on and handled the models. She admits to leaving the empirical social history to others, believing it irrelevant to her explorations of the modern European mind infusing science with gender. Perhaps so, but what is not irrelevant is the reliance of this engendering on codes embodied in texts and artistic traditions. The story she tells is of an elite with shared educational experiences.

The story of the wax models leads Jordanova to an important point in her deconstruction of science/gender. The codes that the viewer-handler learned were not only about the complex male/female binary but also about the uses of realistic detail to heighten the intellectual sexual experience. She calls on the art historian's notion of "hyper-realism" (p. 47) to identify this process by which illustrations in scientific textbooks, like the wax models, both veiled and unveiled the secret of life in the name of scientific clarity and medical progress.

If the actors in her essay on the wax models were a professional elite, then those who appear in her analysis of Jules Michelet are more popular. Michelet's *Femme* (1859) was a text for both a literary

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elite and for a broad bourgeois audience. In this story, we see the incitement of sexuality and the holistic representation of the complexities of gender represented by a master story teller. *Femme* is not the hysterical female deconstructed by Foucault and feminists; it is a far more complex creature of the scientific imagination.

Jordanova traces the story of the scientific construction of "woman as other" from professional wax models to a widely read philosophy of woman. By the time Michelet presented them to a wider public, these were ideas "dense with association" (p. 85). They become even denser and more public in her chapter on the late nineteenth century, which revolves around the public statue "Nature Unveils before Science" made in 1899 for the Conservatoire National des Arts et Métiers. The statue as an explicit parable of valid knowledge revealed to science played on public consciousness of the morality of veils and their incitement of sexuality. Jordanova asks us to see in them as well the metaphor for secrecy and the "fantasy of ownership and display" (p. 96) so delightful and disturbing to this capitalist culture.

Those familiar with traditional histories of science and medicine may follow this juxtaposition of scientific seeking and sexual imagery this far without qualm. The texts she invokes, after all, are peripheral to the esoteric facts that some might call the real history of science. Jordanova makes no epistemological claim on this point, but it is clear from her last two chapters that she does not think that the cultural game of science ended with the rise of technical scientific professions and education. Rather, she shows our continuing addiction to a restless quest for knowledge that simultaneously creates sexual visions and destabilizes those images.

This book does not engage with science as epistemology or as social philosophy. Its greatest failing is its confusion, made necessary by its mission of revealing the subliminal and contradictory images of gender. The book would have been helped by some minimal gestures to social history. By building image on image, however, Jordanova has persuaded us of science's emotional hold on our sexual and natural visions.

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Ian Inkster. *Science and Technology in History: An Approach to Industrial Development*. New Brunswick, N.J.: Rutgers University Press. 1991. Pp. xvi, 391. $50.00.

Debates about the process of industrialization now often focus on the relative importance of the invention of technology, its transfer, and its diffusion. Ian Inkster's argument is that invention counts for less than transfer and much less than getting technology into productive uses, that is, diffusion. What he calls "enclave technologies" are therefore the wasteful villains retarding development. His touchstone for success is not whether a nation's science is superior but whether its technology is used effectively and efficiently.

This argument is ably presented, and on a global scale. The innovators of the eighteenth and early nineteenth centuries, mainly the English and the French, later lagged, in Inkster's view, because they failed to understand that high-tech, high-skill methods would be quick to transfer and slow to diffuse without state intervention. The Germans, Japanese, and Americans surged ahead in the late nineteenth and early twentieth centuries because of their commitment to "industrial drive." Inkster here draws on Alexander Gerschenkron's studies of nations whose drive to "transcend" challenges led to "great spurts" of technological usage. But he also makes the interesting point that in the most successful cases the new science-based educational systems did not destroy the existing base of practical skills that make diffusion easier to achieve in the workplace. Destroying such skills means that the diffusion process will stall, sectoral distortions will generate political opposition, and industrialization will founder.

The chapter on winners and losers takes the argument a step further. Since everything economic and social as well as technical has to be right for true diffusion to take place, there have been many "transfers" but few "transformations." The world's industrial have-nots lagged because of varying combinations of political instability, the loss of "economic sovereignty," and the poor fitting of high-tech, high-prestige solutions to frail diffusion structures. India and China got transfers, mainly Russian and French, but no (or slow) transformations. The chapters contrasting the experiences of Japan, India, and China spell out the differential effects of the variables in detail, as does the chapter comparing United States and Australian development.

In the final chapter, "Twentieth-century Aftermaths: Science, Technology, and Economic Development," Inkster, who also wrote *Japan as a Development Model?* (1980), takes the measure of various nations in terms of their ability to play catch-up and to sustain progress. One of his conclusions is that without state intervention in the face of crises (colonialism, unrest, faulty infrastructure), business people will make decisions adverse to industrialism (export of capital, for instance). Hence the Meiji Japanese strategy of reducing risks (via loan guarantees) and improving infrastructure had—and still has—much to recommend it. He also argues that "If the capital cost of a technology is great, and if the capital cost of controlling social unrest created by its introduction is also great, then the real and opportunity cost of transfer is huge and the process of industrialization will not bear a family resemblance to that of Meiji Japan . . . *Enclave* technology is as wasteful (and wasting) an asset in the twentieth century as it ever was in the eighteenth

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century" (p. 301). Finally, Inkster claims that today's Japan may not have to learn inventiveness to retain its place among the "haves" so long as it continues its superiority in diffusion. Hence the Western nations banking on "best technique" for competitiveness are deceiving themselves.

This work achieves Inkster's aim of taking economies individually so as to avoid the masking of social, institutional, and regional effects with "smoothing" trend-analyses. The book is marred by occasional jargon ("phaseology" for outlining the periods of development), and many readers will no doubt object to the defense of statism as a necessary means of meeting challenges (see p. 170 in particular). Yet in the end this is a useful addition to the study of industrialization because of its attention to all the factors and its global approach.

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MICHAEL BORGOLTE. *Petrusnachfolge und Kaiserimitation: Die Grablegen der Päpste, ihre Genese und Traditionsbildung*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 95.) Göttingen: Vandenhoeck und Ruprecht. 1989. Pp. 430. DM 95.

As Michael Borgolte observes in his introduction, papal tombs are the most visible witnesses of papal history. They are often works of considerable artistic merit and are scattered about Rome, Italy, and southern France. Borgolte is not the first to recognize the importance of papal tombs as historical sources. In 1857, Ferdinand Gregorovius published his enormously successful *Die Grabdenkmäler der Päpste*, in which he tracked the footprints of death to construct a "relief of papal history."

Borgolte builds on Gregorovius, Renzo Montini, and Gerhart Ladner. He systematically explores the questions of where popes were buried, why they or others chose a particular location, and what we may learn about papal monarchy from their choices. He covers the entire history of the papacy, from Peter's putative grave under the present St. Peter's (whose authenticity he doubts) to Pope John Paul I's grave. In a useful appendix he lists the date of death, place of burial, and subsequent translations of all the popes. The book is accompanied by twenty-six plates of papal tombs and ground plans of Old and New St. Peter's showing the placement of papal tombs.

Borgolte's systematic arrangement of the evidence is instructive. In the first three chapters, he describes the uncertain evolution of papal burial practices. Until 217 we do not have any information about the graves of the bishops of Rome. Between 217 and 314, fourteen bishops were placed in a special chamber of the Catacomb of Callistus near the Appian Way. During the fourth and fifth centuries, the popes were buried in various Roman churches, but from the triumph of Christianity until the tenth century Old St. Peter's was the burial place of choice.

In chapters 4 to 7, Borgolte traces the changes of burial practices that followed changes of papal politics. While the Roman nobility dominated the papacy in the tenth and eleventh centuries, St. Peter's lost its place as the preferred burial place of the popes. In the twelfth century, most popes chose the Lateran for their tombs. With the reestablishment of the papal states under Innocent III, some popes were buried in Perugia, Viterbo, and Arezzo. The Avignonese papacy built its commemorative monuments in southern France. From Nicholas V (1455) the popes returned to St. Peter's and have not strayed since. In a final, short chapter Borgolte discusses the papal monuments of the last four centuries.

Art historians have worked on tombs for a long time, and their studies offer interesting evidence about historical self-images. Although Borgolte is not always successful in demonstrating that papal burial practices illuminate the history or ideology of papal monarchy—his subtitle is a more accurate guide to the book's contents than its title—he does offer a panorama that should stimulate historians to consider what they can learn from tombs of popes, kings, nobles, lawyers, and other members of society who had the means to commemorate their deaths.

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ALLEN GUTTMANN. *Women's Sports: A History*. New York: Columbia University Press. 1991. Pp. x, 339. $29.95.

From antiquity (ca. 1500 B.C.) to "Flo-Jo" (1990) in 265 pages is a quantum leap, and tackling the task of documenting the evolution of women in sport for that time span is formidable. Add to that the geographic expanse of the Western world, including New Zealand and Australia, and the job would appear to be virtually impossible. Allen Guttmann's attempt of such an endeavor is to be lauded. For some who have long awaited a definitive and scholarly treatise on the history of women in sport, his volume may be a disappointment. Reviewers such as Richard Mandell have hailed it as "another grand accomplishment." My reactions lie somewhere between these two extremes.

The book is organized chronologically, and early chapters describe the activities of Egyptian, Minoan, and Etruscan women; the Spartan runners of ancient Greece; the Roman gladiators; the sporting activities of medieval women; and raise the question of whether women experienced a Renaissance in sport. The very brevity of these early chapters is indicative of the dearth of information available about those years; yet Guttmann's facility with languages allows him to access resources here and throughout the book that are inaccessible to most.

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Later chapters explore more fully the emergence of women in sport during the Victorian age, the role of schools and colleges in changing opportunities for women, the introduction of Play Days juxtaposed against the rise of the "Muscle Moll" in the 1920s and 1930s, and women's sport under the totalitarian regimes of communists, Nazis, and fascists. Final chapters examine the rapidly changing sport scene following World War II and the revolutionary changes of the 1970s and 1980s. The conclusion focuses on three controversies widely discussed in contemporary literature: women's performance catching up with men, drug use, and the erotic aspects of sport.

Guttmann provides a significant number of examples that substantiate women's participation in sport throughout the Western world and features brief biographical sketches of successful contemporary athletes such as Nadia Comaneci, Billie Jean King, and Wilma Rudolph. For readers with a background in the history of women's sport, much of the information will not be new; the author relies heavily on secondary works and there are some glaring omissions and flaws in describing the development of women's sport in the United States. Gladys Palmer, for example, may have been unsuccessful in establishing a Women's National Collegiate Athletic Association (p. 141), but she is known as the person who initiated the first national collegiate tournament for women (golf) at Ohio State University in 1941, a continuing event that was studied by physical education leaders as the model for intercollegiate competition established for women in the 1960s. The discussion of the Women's Division of the National Amateur Athletic Federation (p. 138), a powerful deterrent to collegiate competition in the United States, ignores the role of Lou Henry Hoover and persons other than women in physical education in the establishment of that organization. And a careful review of resources should verify that an autopsy at the time of her death revealed track athlete Stella Walsh to be a hermaphrodite rather than a man (p. 169).

In spite of such shortcomings, Guttmann is to be commended for a single volume that assimilates a vast array of information about women's sport that heretofore has been available only in widely scattered resources. His interpretation of information is refreshing, and his facility with words makes for interesting reading. For the masses who have little knowledge about the evolution of sport for women, he has provided a yeoman's service.

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COLIN CAMPBELL. *The Romantic Ethic and the Spirit of Modern Consumerism.* (Ideas.) Paperback edition. New York: Basil Blackwell. 1989. Pp. 301.

In this immensely important book, sociologist Colin Campbell sets out to do for consumption and the Romantic ethic what Max Weber did for production and the Protestant ethic. Like Weber's work, this book enables us to see familiar material in new ways, suggests relationships between disparate phenomena, and provocatively explains the origins of our world.

In the eighteenth and early nineteenth centuries, Campbell argues, Romanticism helped facilitate the emergence of modern consumer behavior. When members of the English middle class read novels, intensified their sense of romantic pleasure, and indulged in luxurious consumption, they developed the key characteristics of consumption based on "modern autonomous imaginative hedonism" (p. 77) and the insatiability of wants. What was involved was a series of experiences in which fashion, daydreaming, and the detachment of imaginative pleasure from real products were central.

In explaining how the Romantic ethic led to modern consumerism, Campbell argues for the simultaneous emergence of the Protestant and the Romantic ethics. As Weber did with the Protestant ethic, Campbell gives to the Puritans a key role in the shaping of the Romantic ethic, ascribing to them the bifurcation between feeling and action that made way for hedonism and the belief that the individual could shape emotional experience within the self. Campbell focuses on developments in Protestant (and especially Calvinist) theology in the late seventeenth and early eighteenth centuries that, in elevating the importance of feeling, led the middle class first to the cults of benevolence and melancholy, next to Sentimentalism, then to Romanticism, and finally to Bohemianism. Ultimately, these movements fostered the emergence of an emphasis on hedonism, creative self-expression, restlessness, and emotionalism that undergirded the consumer revolution. Acknowledging that he has argued that consumerism emerged from people who opposed materialism, Campbell explores how ethical ideals can lead to ironic and unintended consequences. Moreover, he believes that at critical moments "self-illusory hedonism" has produced "a genuine romantic idealism" (p. 210). In the end, he concludes, the modern world may well rest on the dynamic tension between the Puritan and the Romantic ethic.

This book makes a number of vital contributions, not the least of which is the nuanced and powerful explanation of the dynamics and distinctiveness of modern experiences of consumption. Offering arguments that are provocative and encompassing naturally involves choices about research methods and intellectual strategies that in turn raise problematic issues. In confining his focus mainly to England (and to a lesser extent to France, the United States, and Germany), Campbell circumscribes the comparative scope of his argument. His analysis rests less on original research into key texts than on how other scholars have analyzed them. Above all, his approach

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is not unlike that of a traditional historian of ideas. At key moments his sense of causation is neither reciprocal nor multidirectional; rather, he often assumes that changes in ideas, values, and attitudes cause shifts in behavior and social conditions. Although he draws on the important work in the social history of consumption, the people who march through the book are philosophers, ministers, and novelists, not consumers, manufacturers, and hawkers.

As they did with Weber's book, scholars will surely try to qualify, prove, and overturn what Campbell has written. Even now, it is clear that Campbell will help set the terms of future debates and has already helped change the way we think about critical elements in the transformation of the past into the present.

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LIONEL FROST. *The New Urban Frontier: Urbanisation and City-Building in Australasia and the American West.* Kensington: New South Wales University Press; distributed by International Specialized Book Services, Portland, Oreg. 1991. Pp. xvii, 226. $24.95.

Lionel Frost has written an important book. In it, the author compares congested cities with more spread out cities. His focus is breathtaking, ranging from Europe through North America to Australia and New Zealand. He finds two urban forms, the highly concentrated city of Atlantic Europe and parts of Australia and the suburban cities of the North American West and the rest of Australia and New Zealand, that is, the New Urban Frontier. He essentially is weighing the claims of suburbia, the dominant form of urbanism in our time. Like many before him, Frost finds suburban cities economically and psychologically deficient. Because of their sprawling character, suburbs are expensive to build. This costs society more money than necessary and the building process discourages industry and nonconstruction employment by outcompeting them for capital. Unlike others, Frost does not place the principal reasons for suburbia in transit, cheap land, the rural ideal, racism, ethnocultural bias, or cheap building materials. Most of these factors matter to Frost, but not crucially. Instead he contends that higher incomes, created by a prosperous "agricultural export-oriented economy," created an economic base capable of sustaining the higher costs of suburban urbanization. In addition, relatively weak immigration to the New Urban Frontier guaranteed that capital did not overwhelm labor and therefore higher living standards prevailed. This is a gifted and challenging hypothesis that deserves to be taken seriously. If validated, it could order a good bit of our thinking about the process of urbanization, urban competitiveness in the new Pacific Era, immigration, labor, cultures, and many other things.

The book is somewhat marred by Frost's ritual bashing of the historical genre of urban biography. Still, the reproof is not to be taken seriously. For the most part, the volume is not based on original research and draws heavily on secondary literature, such as case studies and, of course, urban biographies. It is flawed too by its failure to draw up an adequate balance sheet between the diseconomies of spread cities and those of compact cities. If the suburbs are more costly to build and lonely to live in, as the author claims, the congested cities are also expensive in taxes, disease, traffic jams, pollution, housing prices, and many other things. And the faces in the lonely subway crowd are just as alienated as the suburbanites mowing their lawns or driving their children to Little League. One regrets too the failure to apply this precocious hypothesis to the southern part of the U.S. Atlantic Coast. His explanation of spread cities will find some conundrums there. Southern cities lacked a comparably prosperous agricultural export sector, suffered from low incomes, endured a glutted labor market where labor overwhelmed capital, and still evolved the same sprawling forms of the New Urban Frontier.

Regardless of the outcome of this test, this is a significant book. It applies the comparative method to an extensive historical canvas, presents an imaginative interpretation, and insistently documents the importance of economics to urban history. Inevitably, such sweeping books are hypotheses. This one has given us plenty to think about.

ROGER W. LOTCHIN
University of North Carolina, Chapel Hill

PAUL DUKES. *The Last Great Game: USA versus USSR; Events, Conjunctures, Structures.* New York: St. Martin's. 1989. Pp. xii, 209. $24.95.

The reader who approaches this work expecting to learn something more about the Soviet-American rivalry in Afghanistan or, more broadly, about the Cold War in general will be disappointed. Instead, Paul Dukes argues that the Cold War can be understood and brought to a peaceful resolution only if it is perceived in the most comprehensive frame of reference possible. Dukes takes the work of Fernand Braudel (especially Braudel's more frequently admired than read book, *The Mediterranean and the Mediterranean World in the Age of Philip II* [1972]) as his model. Following Braudel, Dukes attempts to describe the Soviet-American relationship on three levels: "structures" (which include the very different physical and environmental settings of the two powers and the differing human responses to those settings), "conjunctures" (the interplay of middle-range social, economic, and intellectual forces), and finally the superficial, though not unimportant, "events, politics, people."

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This approach leads the reader through discussions of the West German *Historikerstreit* and *Sonderweg* debates, the English civil war and Glorious Revolution, the "Ossian" phenomenon, the poetry of William Blake, and the nature of and differences between modern and contemporary history. Similarly, Charlemagne, Edward VI, Martin Luther, Marshal Vauban, John Smith, and Noah Webster each find a place in Dukes's examination of the Cold War, while Dean Acheson, Anthony Eden, Chiang Kai-shek, Marshal Tito, and Walter Ulbricht do not. Presumably these latter figures can be safely ignored as part of the less-significant "events, politics, people" level of analysis. Political scientists and historians who are not practitioners of the Braudelian "total history" approach will still find interesting discussions of the clash of Wilsonian and Leninist world views, the contrasting postwar convictions of Archibald MacLeish and Andrei Zhdanov, and the interaction of Soviet and American historians at international conferences.

Dukes does not claim that this slim volume constitutes a "total history" of the second Great Game (that would require a massive work at least as substantial as Braudel's *Mediterranean*). It is rather a programmatic exercise intended to suggest new lines of investigation for students of the Cold War. Proponents of the Braudelian approach will welcome this manifesto, while those less sympathetic to the *Annales* school will not find this book particularly rewarding. Dukes, like Braudel, is convinced that looking at history in the *longue durée* will produce more significant conclusions. Unfortunately, as the author admits, "conjuncture and structure" are sometimes "overtaken by events" (p. 161). Recent events have shown that this can happen with almost blinding speed. The author predicts that "although in time [the United States and former Soviet Union] . . . might pass away, or be joined by others similarly qualified, neither of these eventualities is likely in the near future" (p. 90). Those lines, written in 1989, seem to suggest that short-run "events, politics, people" can be minimized only at some risk to the investigator.

TEDDY J. ULDRICKS
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ANCIENT

ROBERT SALLARES. *The Ecology of the Ancient Greek World*. Ithaca: Cornell University Press. 1991. Pp. x, 588. $75.00.

This book originated as a replacement of L. Gernet's work ("L'approvisionnement d'Athènes en blé aux V^e^ et IV^e^ siècles" in *Mélanges d'histoire ancienne*, ed. G. Bloch [1909], 269–391) on the grain supply of Athens, and the original intent survives in the last third of the work, a highly technical discussion of the evolution of cereal crops in the first millennium B.C. In the first two-thirds of the book, however, Robert Sallares applies the principles of demography to numerous topics of interest to the ancient historian.

Sallares's argument depends on the model of an age-class organization of society drawn from comparative anthropology of East Africa and New Guinea. According to his application of this model to Greece, the low population density of the Dark Ages was a direct consequence of the tendency of an age-class system to restrain human reproduction. The adoption and spread of new, more productive crops brought increasing differentiation between rich and poor, however, and provided a new principle of social stratification: wealth. The consequent breakdown of the age-class system removed its restraint on reproduction and was responsible for the population explosion of the eighth century (he argues for A. M. Snodgrass's estimate, in *Archaic Greece: The Age of Experiment* [1980], of a 3 to 4 percent annual growth rate) as well as for a multitude of other events, including a transition from bridewealth to dowry, the conspiracy of Kylon, the origins of slavery, and even the origins of the polis.

The evidence for such an age-class organization in Greece is elusive, however, and its effect, if it existed, seems confused. Sparta and Crete offer the *agoge*, pederasty, the *syssitia*, and the evidence of grave assemblages from sub-Minoan Knossos differentiated by age and sex (A. J. M. Whitley, *Style and Society in Dark Age Greece: The Changing Face of a Pre-literate Society, 1100–700 B.C.* [1991]). But in Crete there is a shift from heterogeneity to homogeneity in burial assemblages from the tenth through the eighth centuries, which reverses the trend in Attica from the homogeneity of Protogeometric to the heterogeneity of Late Geometric burials (Whitley, pp. 188–89). If the type of shift found in Attica is indeed indicative of an end of an age-class system and its restraints on population, and ultimately the cause of the rise of the polis, one wonders how to account for the polis in Crete and elsewhere in Greece.

Some of the forays that Sallares makes into questions of general history will directly interest historians, who are likely, however, to make short work of arguments such as that the plague described by Thucydides was smallpox or that the *hektemoroi* gave five-sixths of their produce. But the primary value of the book for the historian is to raise new issues and, in particular, to suggest the potential contributions of demography and ecology to the investigation of historical questions. The book's generous footnotes and bibliography will be especially helpful in leading the reader further into such issues.

The reader should be forewarned, however, that Sallares sometimes deals imaginatively with his sources. A good example is the material that he cites to support his claim that Aristotle used age as the main defining principle of the polis; while one can see how he reached this conclusion, not everyone would be willing to accept it as Aristotle's opinion. Some-

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times, moreover, in the enthusiasm of an argument Sallares converts the conclusions drawn from the application of a model into facts. But the cautious reader will nonetheless profit from the new perspectives on historical questions that this stimulating book offers.

NANCY DEMAND
Indiana University, Bloomington

GORDON S. SHRIMPTON. *Theopompus the Historian*. Buffalo, N.Y.: McGill-Queen's University Press. 1991. Pp. xviii, 346. $49.95.

Theopompus was one of the most productive and significant of the Greek historians of the fourth century B.C. His works heavily influenced later historians. Yet despite all of his efforts, not one work has survived intact. Thus, Gordon S. Shrimpton confronts the challenge of understanding the historian and his work from a scant 409 fragments.

In chapter 1 of this volume, Shrimpton discusses Theopompus's life, works, and style, while in chapter 2 he explains how he used extant fragments to reconstruct lost works. When he considers the sources that Theopompus used, he deals in pure speculation, and he could have devoted more attention to Theopompus's originality. In working with the fragments themselves, he uses the sound method of fitting them into a context provided by other ancient historians. This approach is necessarily and frequently conjectural, for so much has been lost that certainty is unattainable, as Shrimpton realizes. The *Hellenika* is an excellent example of the problem. Theopompus wrote the *Hellenika* in twelve books, of which only nineteen fragments survive. It is thus patently impossible to reconstruct the work.

Theopompus's greatest work was his mammoth *Philippika*, written in fifty-eight books that treated Greek affairs and the career of Philip II of Macedonia. Shrimpton makes a good attempt in chapter 3 to establish the organization and contents of the work, but he admits that the task is impossible. He emphasizes Theopompus's treatment of Athenian demagogues, the successful revolt of Byzantion from the Second Athenian Confederacy, Greek relations with Persian satraps from circa 394 to the mid–340s, and various events in Philip's career.

In chapter 4 Shrimpton uses the fragments to interpret Theopompus's moral and political views, which he admits are beyond recovery. Only in the next chapter does he reach the heart of the political matter of the *Philippika*. He gives a brief description of the latter half of the fourth century B.C. Theopompus's presentation and interpretation of events could be better understood if discussed in the forefront of an accurate historical background. Shrimpton concludes that Theopompus was favorably inclined toward Demosthenes because of his self-control and opposition to Philip. Philip was bestial, a victim of his own worst desires. He was not even a good general, for he simply enjoyed the good fortune to conquer a weakened Greece. The book concludes with two appendices, one on the authorship of the *Hellenika Oxyrhynchia*, in which he cautiously suggests that Kratippos, of whose history only three fragments survive, was the author. Appendix B provides a translation of Theopompus's fragments.

Shrimpton has made the most of a singularly intransigent topic. His approach is sane and circumspect. The major weaknesses of this book are its many historical errors and its reliance on older bibliography. Nevertheless, these problems do not overly detract from the value of the book.

JOHN BUCKLER
Universität München

CHARLES D. HAMILTON. *Agesilaus and the Failure of Spartan Hegemony*. Ithaca: Cornell University Press. 1991. Pp. xix, 280. $37.95.

As any reader of Xenophon's *Hellenica* quickly learns, the early fourth century is a labyrinth of people, events, and societal change. Even attempting to navigate the maze necessitates a strong rope to serve as guide. Charles D. Hamilton's rope is the career of Agesilaus, king of Sparta from 399 to 359/8. The intertwined fates of Agesilaus and Sparta to 362 anchor a discussion that is intended for general readers as well as scholars.

Beyond providing a focus for this chaotic period, the topic offers excellent scope. Earlier events, basic institutions, and the affairs of other states that touch on Spartan affairs form a natural part of the narrative. Not simply biography or analysis of political/military events or historiography, the book combines all three genres in examining the nature of Spartan hegemony and its failure.

Much of the book draws on careful analysis and comparison of the sources to give a chronological narrative of events during this time. The book differs from other recent interpretations in arguing for Agesilaus as the shaping force in Sparta's role in these events. Proceeding from the premise that the individual, rather than inanimate or collective forces, can be the shaping force in history, Hamilton believes that Agesilaus is not only "the single most important individual in the period" but also the man whose "policies led to Sparta's failure" (p. x). To understand those policies, moreover, requires probing the king's inner being. Here Hamilton looks to psychohistory to uncover Agesilaus's thoughts and motives.

Hamilton is moderate in arguing these premises. His Agesilaus is believable and he does not ignore the society that conditioned Agesilaus. Still, the reconstruction must necessarily remain speculative, even with the relatively full evidence we possess. A certain delineation of inner being is impossible when we lack

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the subject and the subject's own words. The uncertain portrait is thus an insecure foundation for conclusions about Agesilaus's direction of Spartan policies. Yet, after reconstructing Agesilaus's probable boyhood traumas, Hamilton passes to conclusions based on them.

Equally problematic is the interplay between individual and society. As king Agesilaus had certain prerogatives and, with his considerable powers, was at the center of a particular policy, as were many Spartan kings. Kingship, however, was also constrained; the Spartan state in general was organized to subordinate the individual to the community. Hamilton's narrative aptly demonstrates the fate of Spartan leaders who did not conform to expectations or limitations. If Agesilaus had controlled Spartan policy as thoroughly as Hamilton claims, the Spartan failure was far earlier and more dramatic than we have supposed. Hamilton's account also shows that Spartan policy was affected by actions of many other states over which Agesilaus had no control. The importance of Agesilaus is undeniable. Nevertheless, he was not a free agent.

But the period is unintelligible without a focus. The disorder of the times depressed even Xenophon. After slogging through to Mantineia, he ended his account with the sad words "there was even more uncertainty and confusion in Greece after the battle than there had been previously" (vii.5.26). In full command of the evidence, Hamilton has been selective with the massive detail, and his habit of repeating major points adds clarity to the larger picture. Nor is the author infatuated with his subject, recognizing weaknesses as well as strengths in both Sparta and Agesilaus. The psychological analysis may not convince but it does not impede our appreciation of his detailed and lucid account of this important period.

Carol G. Thomas
University of Washington

Emilio Gabba. *Dionysius and* The History of Archaic Rome. (Sather Classical Lectures, number 56.) Berkeley and Los Angeles: University of California Press. 1991. Pp. xviii, 253. $35.00.

In his ninth book, Livy devoted an excursus to Alexander the Great that was intended to refute the subversive ideas of unnamed Greek historians who sought to denigrate Rome. Livy's contemporary, Dionysius of Halicarnassus, referred to the same hostile Greek views in his poem and undertook to correct them by producing the first accurate history of Rome written in Greek. This was not the statement of an unduly modest man, for the earliest histories of Rome were, in fact, written in Greek, albeit by Romans, and Dionysius's statement is a measure of their standing in the Greek literary world. But Dionysius had a mission: to explain the rise of Rome to world dominion in a way that the Greeks would find palatable. He claimed that Rome was a Greek city to begin with.

Dionysius's *Roman Antiquities* has not been altogether neglected, but neither do modern scholars rank it high. Emilio Gabba attributes this to a damaging assessment of Dionysius written by E. Schwartz in 1905. Schwartz considered Posidonius the last great Greek historian, and his successors, of whom Dionysius was one, to be lifeless imitators of worn out models. Gabba undertakes redress: Schwartz, he claims, completely misunderstood Dionysius.

Gabba looks first at the background: the revival of classicism in the Greek world under Augustus. Plutarch suggested that there was something dangerously separatist in glorifying the Greek past. Dionysius, however, believed the Greek past should be exalted, but within the framework of the Roman empire. Next follow chapters on Dionysius's methods and his antiquarianism, where Gabba makes a number of interesting points. He notes Dionysius's connection, or lack of it, with Livy, and compares Varro, who had also insisted on the ancient tie between Greece and Rome. He contrasts Thucydides's *Archaeology* with Dionysius on early Italy, and he remarks shrewdly on Dionysius's treatment of Heracles as a bringer of civilization. Here there are also comparisons to be made with Vergil's *Aeneid*, although Gabba neglects them. On the question of Aeneas himself, Dionysius confidently identified the gods he brought to Italy: they were the Great Gods of Samothrace.

Two final chapters deal with the social and political structures of early Rome and the political implications of the *Roman Antiquities*. Rome prided itself on its capacity for assimilation. This was not a trait of the classical Greek city-state and what Dionysius thought of it is not clear. But he saw Rome's early political struggles as an essentially peaceful evolution.

When Dionysius wrote of Rome's archaic period, however, he was intent on the Rome of his own day. Gabba compares him with Josephus, who made the *Roman Antiquities* the structural model for his *Jewish Antiquities*. The comparison has only this much validity: both historians wrote with the aim of finding an accommodation for their people within an ascendant Roman empire. Yet Gabba's book is valuable for its insights and thought-provoking ideas, even where one hesitates to agree.

J. A. S. Evans
University of British Columbia

MEDIEVAL

Helen Clarke and Björn Ambrosiani. *Towns in the Viking Age*. New York: St. Martin's. 1991. Pp. xii, 207. $39.95.

This book by Helen Clarke and Björn Ambrosiani is a new synthesis of information from archaeological investigations of early medieval towns in Northern

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Europe, from the late Roman period to the eleventh century. Its geographical coverage is broad, from Ireland in the northwest to Novgorod and Kiev in the east. The focus is on specific town sites that have been explored archaeologically, and the text provides detailed description of important finds and assessment of their significance. By "town" the authors mean a settlement whose inhabitants live primarily from commercial activity, not food production. The emphasis of the book is on Scandinavia and the southern Baltic coast, areas not well covered in recent English-language studies.

The authors begin with questions of continuity in town life from late Roman to early medieval times. To illustrate the resurgence of towns in the seventh century, they summarize recent excavation results from London, Dorestad, Ipswich, and Southampton. In the eighth and ninth centuries, these towns grew, and others arose.

The longest of the six main chapters concerns northern Germany and Scandinavia, where archaeological research has been particularly active on Viking period towns. Evidence from Haithabu/Hedeby in Germany; Ribe in Denmark; Helgö, Birka, Sigtuna, and Löddeköpinge in Sweden; and Kaupang in Norway, is yielding rich new information about town development, trade and industry, and cultural change. A chapter on Britain details the growth of such towns as York, Lincoln, and Dublin. In their treatment of the south and east coasts of the Baltic, the authors review results of often extensive excavations at Ralswiek on Rügen and Wolin at the mouth of the Oder, as well as inland sites such as Staraya Ladoga, Novgorod, Smolensk, and Kiev.

A synthetic chapter draws together many of the themes and addresses such topics as locational types, travel and trade, industry, town administration and layout, and cemeteries. The book demonstrates the striking similarities among towns of this period in Northern Europe, from Dublin to Kiev. Many share common types of location, street layout, house type, and manufacturing activities. Similar trade goods are found at many of the towns. A brief final chapter offers a thoughtful discussion of ongoing research and of issues that merit further investigation.

This is an important book for medieval historians and archaeologists. Archaeological research is yielding vast amounts of new information about town development, industry, trade, and demography in Northern Europe, where written sources are often scanty. The authors place the archaeological evidence in its historical context, bringing into the discussion events and documents relating to the towns. The copious footnotes and extensive bibliography provide the interested reader easy access to the wider literature. The book is extremely well-illustrated with regional maps, site plans, aerial photographs, and drawings of important objects. This book should be required reading for both specialists and students concerned with the early medieval period.

PETER S. WELLS
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J. F. HALDON. *Byzantium in the Seventh Century: The Transformation of a Culture*. New York: Cambridge University Press. 1990. Pp. xxiii, 486. $69.50.

After a brief preface on sources that looks like an afterthought, J. F. Haldon warns readers in his introduction that "this book is conceived and written within a historical materialist framework—that is to say, it is written from a 'Marxist' perspective" (p. 6). As for sources, "there is no use in appealing to an objective, fact-based history, for such does not, and indeed cannot, exist" (p. 7). What follows is a lengthy account of Byzantium from 610 to 717 based overwhelmingly on secondary literature, with occasional references to "K. Marx" as to an oracle (for example, p. 101 n. 18, p. 452 nn. 15–16).

Haldon nonetheless summarizes the work of some less-dogmatic Marxists and non-Marxists, particularly A. H. M. Jones (chap. 1), Andreas Stratos (chap. 2), R.-J. Lilie (chap. 3), Michael Hendy (chap. 5), J.-L. Van Dieten (chap. 8), and Friedhelm Winkelmann (chap. 10). But Haldon implies that these scholars agree with him and with each other more than they actually do, and he makes some major errors while summarizing.

He states that in 602 Slavs held most of the Balkans (p. 35), although Michael Whitby's recent book on the emperor Maurice (*The Emperor Maurice and His Historian* [1988]), uncited by Haldon, has shown the contrary. He refers to a determined Arab siege of Constantinople in 674–78 (pp. 63–64), but Paul Lemerle's study of the miracles of St. Demetrius (*Les plus ancien recueils des miracles de Saint Demetrius et la penetration des Slaves dans les Balkans* [2 vols., 1979, 1981]), cited by Haldon elsewhere, has found only Arab raids around the city. Haldon declares that until 681 Slavs "recognized Byzantine overlordship" in "part of the Plain of Walachia" and "south Bessarabia," yet no source suggests anything of the kind (p. 67). A list he provides indicates that the Arabs took Smyrna three times; no source says that they took it even once (p. 107).

The rest of the book is mostly theorizing by Haldon, much of it perfunctory and unilluminating. For example, after criticizing previous scholarship for concentrating on what the Byzantines believed rather than why they believed it, he offers this sort of Marxist boilerplate: "Within the symbolic universe and drawn from a number of its elements, specific ideological discourses are generated, chiefly a result of the contradictory nature of the social relations of production (that is to say, through the existence of antagonisms between different social groups in re-

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spect of their relationships to the means of production and distribution of wealth, to the sources of power and so on: antagonisms which need to be made sense of)" (p. 325).

The real point of the book comes in chapter 6, on military administration. Haldon accepts Michael Hendy's argument that military pay must have fallen below subsistence level, since after the loss of Egypt and Syria government revenues plummeted while the army remained large (*Studies in the Byzantine Monetary Economy, c. 300–1450* [1985]). But he dismisses the traditional view, adopted in a sophisticated form by Hendy, that from the mid-seventh century the empire supported its soldiers by grants of land. Instead he hypothesizes that "the state returned to the system of the fourth century," levying supplies from taxpayers and distributing them to soldiers free of charge (p. 241). Mostly on the strength of this theory, he asserts in his concluding chapter, "More than anything . . . the seventh century marks in many respects the reassertion of imperial authority and the power of the state over its resources" (p. 449). Thus, Byzantium became a "feudal" society, but one of a sort unknown to Marx or anyone else: in it surplus production was appropriated not by nobles but by the state (p. 454). The strengthened state saved the empire.

This seems strange Marxism and bad economics. The fourth-century empire collected and distributed military supplies in kind not because it lacked resources but because its currency had collapsed. The resulting system wasted resources in shipping supplies all over the empire, and when Anastasius I monetized taxes and payments he saved enough to cut taxation and amass a large reserve. Since in the seventh century the currency remained sound, attempting to pay soldiers in kind would simply have aggravated an already desperate fiscal crisis.

The obvious alternative of the military lands appears to be incompatible with Haldon's Marxism because it would have involved distributing crown land to the soldiers, meaning that the empire was saved not by more state control but by less. Yet Haldon has failed to develop a credible alternative to this theory, or indeed any plausible view of seventh-century Byzantium. Instead these almost 500 pages, seemingly addressed only to those who share his preconceptions, offer a rehash of twentieth-century secondary literature from a nineteenth-century viewpoint.

Warren Treadgold
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Valerie I. J. Flint. *The Rise of Magic in Early Medieval Europe*. Princeton: Princeton University Press. 1991. Pp. xii, 452. $35.00.

In this provocative book, Valerie I. J. Flint argues that magic became an integral part of early medieval Christian culture through a conscious and deliberate "rescue" of pre-Christian beliefs and practices. Recognizing "the emotional need for that magic which sustains devotion and delight" (p. 4), early medieval clerics carried out this rescue by developing acceptable forms of Christian magic to replace unacceptable forms of control over the irrational. In opposition to the notion that Christian leaders were unable to fend off the primitive superstitions of the peoples among whom they worked, Flint argues that they actively refashioned Christianity to assure its penetration into societies quite distinct from the antique Mediterranean world of its origins. Thus, the author wants to perform another sort of rescue: defending early medieval churchmen from the charge that they failed to keep Christianity free of superstition and barbarism. She hopes to counteract the "colonising" tendencies of modern scholars who have assumed that people in less-developed times must have been "less subtle, even less humane than ourselves" (p. 394). Insofar as she succeeds, she sheds new light on one of the great success stories of history, the Christianization of Europe.

The introductory chapters discuss Roman and early Christian attitudes toward magic, survey the early medieval sources, and raise the central issues of the book: the how and the why of the accommodation of magical practice and belief in early medieval Christianity. The rest of the volume is organized into three parts of two chapters each, treating in turn the magic of the heavens, the magic of the earth, and magical practitioners. In each part, the author presents first the magic that early medieval churchmen opposed and then that which they accepted, transformed, and, occasionally, invented. For example, while the practice of divination was everywhere condemned, Christian forms of astrology were not, especially in medical practice, because their acceptance blocked other less-savory practices. Similarly, angels were made to absorb the powers of good *daemones* in the fight against unredeemable evil ones. The bibliography lists both Latin editions and English translations of primary works when available, and is thus a valuable tool for teaching and undergraduate research as well as a rich source for further reading. The index is a complete and generally reliable guide to the many topics covered.

Flint challenges historians to treat early medieval religion on its own terms, and her detailed discussions of particular topics are unfailingly interesting. I remain, however, unconvinced of the book's thesis. The argument for conscious and deliberate action by clerics is too often made by affirmation rather than by evidence. Flint's bishops and abbots sound suspiciously like late-twentieth-century dream images of post-colonialist missionaries, gently facilitating rather than forcing acculturation. Above all, Flint's strong rhetorical stance forces her to replicate both the qualities and the categories of the view that she opposes. Thus, her representation of both Christian-

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ity and the church is no less one-sided than its opposite. While traces of the attitudes about early medieval Christianity to which she so strongly objects may indeed appear in print from time to time, little is gained by constructing an equally simplistic alternative. The best recent work in the field situates change and continuity of Christian belief and practice within the social, political, intellectual, and cognitive structures of individuals and groups at particular times and places. Flint's book is a valuable contribution to the research program implied by such work, but more for its richness of detail than for its overarching theme.

FREDERICK S. PAXTON
Connecticut College

BENJAMIN ARNOLD. *Princes and Territories in Medieval Germany*. New York: Cambridge University Press. 1991. Pp. xiv, 314. $54.50.

After focusing on the lower nobility in his *German Knighthood 1050–1300* (1985), Benjamin Arnold has now turned to the high aristocracy. He offers an answer to the question: "why did Germany evolve into a multiplicity of autonomous states under secular dynasties, urban authorities, and prince-bishops, becoming a species of aristocratic congeries in which the crown enjoyed enormous prestige but minimal authority?" (p. 1). The formation of the territorial lordship was not the result of conflict between the crown and the princes, the princely usurpation of royal rights, or even the failures of individual rulers, but was due rather "to the savage energy and forceful personality traits" (p. 5) of the princes. Arnold stresses that the kings, who lacked the means to govern their diverse country, left the exercise of regional authority to the princes, with their inherited right to rule.

The princes were the chief promoters of the economic expansion that transformed the German landscape and extended the country's frontiers eastward between 1050 and 1300. Although they showed great flexibility in encouraging both internal and external colonization, their right to promote land clearance was based on their traditional dominion over waste land and manorial jurisdiction. Indeed, Arnold questions whether the alleged "rise of territorial lordship" (*Landesherrschaft*) during the High Middle Ages can be employed as anything but a useful description for the phenomenon of princely power. Lordship had contained a territorial element since the ninth century, and it continued to depend even in the fourteenth century on such things as the command of armed retinues.

Arnold emphasizes in this unabashedly constitutional history the element of continuity. Like his mentor Karl Leyser, Arnold is skeptical about the supposed change from consanguinity to patrilineal dynasties in the structure of the nobility. It may be that the adoption of toponyms was simply a more accurate way to designate the members of a greatly enriched elite.

If there was a new component to princely lordship in the twelfth century, it was the reform of regional jurisdiction. The crown assigned to the princes the duty of enforcing the peace. The result was the creation of regional magistracies (*Landgerichte*) with the power to impose capital punishment on a wide variety of social groups. These were embellished with the prestigious title of counties, usually associated with one of the new stone castles from which the princes exercised their traditional lordship over men and land.

This provocative book is based on an impressive knowledge of the sources and literature, but a bibliography would have made it easier to use. I have some methodological and interpretive reservations. In spite of numerous specific examples and an excellent concluding survey of the political topography, Arnold's approach obscures the differences in regional development. I wish that he had examined in greater detail the history of specific principalities. Why, for instance, were the dukes of Carinthia so notably less successful than their Styrian neighbors in establishing their dominion? Beyond that, there remains the unmentioned example of France. Surely the dukes of Normandy and counts of Anjou were also forceful personalities, but it was the Capetians who were the chief beneficiaries of princely "savage energy." This suggests that we cannot overlook, among other things, the German kings' entanglement with the papacy, the triumph of the elective principle, and such dynastic accidents as Henry VI's early death in answering Arnold's question.

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SALLY THOMPSON. *Women Religious: The Founding of English Nunneries after the Norman Conquest*. New York: Clarendon Press of Oxford University Press. 1991. Pp. 314. $79.00.

Sally Thompson's decision to focus her book on foundations confines her to a study of numerous English female communities smaller and poorer than the ten surviving Anglo-Saxon abbeys. They are traced to three sources: anchoresses loosely attached to hospitals or to monkeries; expanding Continental orders; and lay or episcopal founders. As she herself notes at some length, most of these foundations lack formal documentation and are often not even alluded to in the literature. Accordingly, her extensive archival research only meagerly enhances literary evidence like the *Life of Christina of Markyate* (1987). There is also considerable padding in her book, as in the repetition of the latter biography, even though it omits any reference to the convent's foundation. Similarly, the Continental history of Fontevraud does

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not help to document the foundation of Fontevrist houses in England. While sympathizing with Thompson's frustrations as a researcher, I cannot help but wonder why she chose to study an aspect of monastic life that yielded such poor return.

The "post-conquest period" is rather clumsily dated from 1100 to 1250. Thompson provides little background information about the effect of conditions in Norman England on the religious life of women. She does track the increasing hostility of monks to the *cura mulierum* and notes a general trend for initially two-sex communities to separate and create nunneries. Although Thompson traces Continental developments, she makes no systematic effort to link them to English chronology. Instead, her decision to treat the Gilbertines in a discussion of "double monasteries" in England and to relegate the similar development of Fontevraud and other syneisactic communities to another portion of the book clouds the main issue of this ongoing separation of men from women.

Thompson's point of view regarding this development is not very clear. Is she being ironic when she characterizes (p. 134) the Cistercian refusal of women as "a wise prohibition"? Or when she describes (pp. 134–35) the notoriously misogynistic Premonstratensian abbots as "admitting the failure of the double monastery"? She appears to take Jacques de Vitry's estimate of "increasing moral danger" (p. 135) literally and to agree with twelfth-century monks that the "problem" of "support for the nunneries" (p. 157) could not be solved by the association of women and men. The contrary evidence of the Fontevrists, where the abbess had the authority to assign the monks to the care of the nuns, is dismissed as a failure because of continuing friction in the order.

A separate section is devoted to the Continental orders whose extension to England was only a peripheral experience. Thompson has subjected the evidence relevant to each of these foundations to careful scrutiny. The result, however, is failure to settle a question that in fact may have no impact on the female experience: to what male order were English nunneries attached? This male-centered organizing principle fragments and marginalizes nuns; similarly, this book never finds a coherent focus in the particular economic or spiritual needs of its subjects.

Clarendon Press is to be thanked for respecting old ways while introducing a welcome innovation: notes appear at the bottom of the page and bibliographic citations conform to abbreviations used in the notes.

Jo Ann McNamara
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Stephen E. Wessley. *Joachim of Fiore and Monastic Reform.* (American University Studies, seventh series; Theology and Religion, number 72.) New York: Peter Lang. 1990. Pp. xii, 148. $39.95.

In 1188, Joachim received papal permission to leave the abbey of Corazzo, of which he had been abbot. He had lived in a hermitage since 1185, but now he journeyed into the Sila Mountains, where he founded S. Giovanni in Fiore. This, the motherhouse of the Florensian Order, had gained considerable prestige by 1250. Joachim never mentioned the Florensian Order, yet scholars have surmised that it must have had a role in his expectations of a coming *status* of the Holy Spirit. Stephen E. Wessley's book investigates the role that the Florensians believed Joachim intended for them and the extent to which the Florensians were active in propagating Joachim's thought and writings.

Wessley relies principally on Joachim's *De vita s. Benedicti et de officio diuino*, the *Vita b. Joachimi abbatis* (which Wessley argues was written by a Florensian who had been Joachim's companion since the abbot left Corazzo), the *B. Joachimi Abbatis miracula*, and the *Super Hieremiam*. The last two have only recently begun to receive the critical study they deserve. The Florensian biographer who wrote *B. Joachim Abbatis miracula* in about 1209 depicted Joachim as a new Moses who had been sent to lead the monastic *viri spirituales* from the corrupt world to the new land of promise, Calabria. S. Giovanni in Fiore was the new Nazareth, where in a life of strict poverty, chastity, and austerity they would create the "Jerusalem" of the incipient third *status*. Wessley correlates this "exodus" interpretation with Joachim's *De vita s. Benedicti*, where Joachim used Benedict as a model to justify his role in leaving Corazzo and founding S. Giovanni (pp. 29–48). Wessley suggests that Joachim saw himself as an Elijah figure whose mission was to inaugurate the monks of the third *status*.

Wessley follows Robert Moynihan in arguing that the manuscript tradition indicates that the *Super Hieremiam* had a complex development. He is convinced that the work was either Florensian or Cistercian in origin, not Franciscan. Both it and the *Miracula* are, he argues, excellent evidence for the Florensian reaction to the condemnation of Joachim's book against Peter Lombard in 1215, and for their continuing image of themselves.

Wessley has made excellent use of the recent manuscript research by Fabio Troncarelli and A. M. Adorisio on codices from S. Giovanni and the Cistercian/Florensian monasteries of southern Italy. His thesis is important and on the whole convincing. Students of Joachim, scholars interested in the Cistercian Order, and even those interested in religious reform in this period will find this a book worth reading carefully.

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Karl F. Morrison. *History as a Visual Art in the Twelfth-Century Renaissance.* Princeton: Princeton University Press. 1990. Pp. xxvi, 262. $35.00.

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Karl F. Morrison has written what can best be described as a meditation on medieval culture. His book has a discursive rather than analytical organization, flowing from one observation to another rather than rigorously developing conclusions that scholars could use as points of departure. One is forcefully reminded that English-speaking historians are far more reluctant than Continental historians to describe their methods as scientific or to see themselves adding to a body of knowledge. Moreover, the subject matter is hard to pin down. The title derives from Morrison's argument that medieval historians shared the impulse of visual artists to make their work vivid, but this is not so much argued as asserted; there is practically nothing on the visual arts, while the written sources discussed are more frequently theological than historical in content. The time period from which Morrison draws his material is similarly imprecise; to judge from the material discussed, the twelfth-century renaissance runs from the eighth century more or less to Dante and Chaucer. Oddly for a book on intellectual history covering this period, the revival of learning, foundation of universities, and rediscovery of classical antiquity play no part at all.

The book begins with some observations about method developed in the form of an imagined dialogue with a diverse cast of participants including Archbishop Adalbert of Hamburg-Bremen (1043–72), Villard d'Honnecourt, John of Salisbury, Rabelais, Gadamer, and Dante; the subject of the dialogue —metalinguistics, the ambiguities of interpretation, and the possibility of knowledge distinct from language—clearly situates it in the environment of 1980s literary criticism, and it is consistent with the spirit of that time that no conclusion is reached. The remainder of part 1 consists of three chapters. In chapter 2, on history as an imaginative art, Morrison argues that the lack of narrative structure apparent in most medieval histories results from the fact that historians generally saw their art as consisting of the vivid presentation of individual scenes; in effect, they aspired to translate visual images into language. In chapter 3 he argues that religious ideas shaped the choice of subjects for medieval writers and artists, and in chapter 4 he posits that mimetic or non-linear thinking, common in the tenth and eleventh centuries, became less common after the twelfth century; rather oddly, Morrison's principal example of mimetic thinking is the late twelfth-century *Hortus Deliciarum*, while a few cursory remarks about Dante, Chaucer, and Cimabue suffice to describe the subsequent "change of cognition."

Part 2, which focuses more closely on works that are historical in a strict sense, is devoted to silences of various kinds: about the Kingdom of God (chapter 5), women (chapter 6), and time (chapter 7). Morrison described these silences as "esthetic," the point being that historians omitted material that properly belonged to non-narrative genres.

Morrison claims as one of his guiding principles that "art's coherence was not in the work, but rather in the actual processes of esthetic judgment by which it was composed, and in the responses of the intended audience" (p. xv). But he appears to have little idea of how to put this principle into practice.

Morrison notes, for example, Matthew Paris's dependence on Roger Wendover's chronicle for the earlier portions of his own history; but it apparently did not occur to him to use Paris's revisions of Wendover's account to illustrate his argument that historians aspired to visually expressive retellings of events. Indeed, individual people or works are remarkably absent from this work of intellectual history, for Morrison's usual method of presentation is to cut back and forth between works belonging to different genres and authors belonging to different periods. I noted sustained discussions only of Bede (pp. 56–66), John of Salisbury (pp. 79–90), the *Hortus Deliciarum* (pp. 112–20), Cosmas of Prague (pp. 168–69), and Gunther of Paris (pp. 230–39). This last passage, in which Morrison shows that the demands of narrative poetry made Gunther reshape his prose sources, both illustrates what is missing elsewhere in the book and calls into question the loose conflation of genres on which Morrison's presentation depends.

Although this volume is impressive as a display of learning, Morrison's methods unmistakably suggest that he regards all medieval authors and artists as more or less interchangeable. Any gap in one source can be filled by another, for all are equally representative of a medieval culture that is not only Christian but also specifically meditative and monastic; lay culture, even universities, are absent, as is any hint of politics. Such an approach would be unacceptable for any period more modern than this one. It is hard to see why medievalists should take it seriously either.

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GÉRARD SIVÉRY. *Terroirs et communautés rurales dans l'Europe occidentale au Moyen Age*. (Économies et sociétés.) Lille: Presses Universitaires de Lille. 1990. Pp. 248. 120 fr.

The central thesis of Gérard Sivéry's study is an attack on the tyranny of the construct of seigneurialism in medieval history. Historians have been too faithful to the preoccupations of Old Regime scholars, he writes, and have looked at the sources from the viewpoint of fiefs and lordship. His own study of franchise charters and other fiscal and administrative documents, presented more fully in his *Structures agraires et vie rurale dans le Hainaut à la fin du Moyen Age* (1977–80), discovered many rural communities that enjoyed considerable autonomy. As he puts it in the introduction to this book: "Peasants were themselves organizing different aspects of their common life to a greater or lesser extent, with scant regard for lordship" (p.

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10). The charter of Prisches (1158), in Hainaut, for example, which accorded a large measure of administrative and judicial autonomy, including the power of life and death, served as a model for about thirty others both in the kingdom of France and the empire. Such documents depict rural communes and federations of villages that elected their own *echevins*, managed their own finances, exercised the rights of low and in some cases of high justice, and successfully resisted, on occasion, the efforts of nearby lords to restrict their liberties. This is only part of the story, of course. Many village charters reserved extensive rights to lords, and many more villages were without charters at all. In Sivéry's terms, *terres de libertés* in some areas were opposed to *pays de soumission* in others. These differences are explained by a mixture of historical, topographical, and economic considerations. The fundamental distinction between "old territories" (*vieux pays*), cultivated as part of the Carolingian estates and even earlier, and lands cleared during the eleventh through thirteenth centuries (*pays neufs*) is well known. The former were generally sited on limestone plateaus that could be cultivated with light scratch ploughs and produced primarily cereal crops. The *pays neufs* cover a variety of activities besides the familiar working of heavier clay-based soils with the new wheeled ploughs, stockraising, or vineyards. This greater diversity in the use of the *terroir* increases later in the Middle Ages and correlates, Sivéry argues, with the peasants' ability to define and defend their interests and to adapt to the market economy. This often involved strategic compromises with the claims of lordship. Sivéry proposes a model of organization he calls *pays de transition*, which he thinks ends up comprising a majority of cases for the areas west of the Rhine and north of the Alps. Part of the village *terroir* would be made up of lands cultivated since the earlier Middle Ages, where some lordship rights were acknowledged, but on lands more recently developed (*pays neufs*), they were not.

The densest part of his analysis focuses on the region of Hainaut that he has been studying since the 1950s, the *terre de Guise* and surrounding region east of Cambrai. Many Carolingian domains (and Roman villas, for that matter) are located on the limestone plateaus around Cambrai and here, in the Middle Ages, lordship rights were strong and well maintained, village charters were few, and open field cereal agriculture came to predominate (*pays silencieuses et soumises*). To the north of the River Oise lay the lands of the former forest of Thièrarche, mostly developed during the high medieval land clearances (although some villages go back to earlier settlements in forest clearings): here charters and autonomous rural communities abounded, lordship rights were few or vigorously contested (the Abbot of Maroilles kept trying to get his acknowledged), and some cereal cultivation was mixed with a lot of stockraising. Farther east were the *confins ardennaises*, *pays neufs* where the soil is poor, the dense forest is nearby, and, although one does find written charters often enough these tend to reflect the banal rights of the numerous small-scale feudal lords. The relative strength or weakness of lordship from region to region depends on a number of variables. The strongly autonomous charter of Prisches, for example, was backed by the lords of Avesnes, holders of public powers in the region, who supported the villagers against efforts to impose certain local lordship rights around 1230. As time went on the greater lords, the king of France and the count of Hainaut, in the process of reasserting their regalian rights, preferred strong rural communities to strong banal lords. But even in regions where lordship was strongest one should not assume, Sivéry warns, that the peasant communities were helpless because they stuck to custom and oral tradition instead of charters. A case can be made for the greater malleability of non-written custom. Strong lordship, too, might well serve some community purposes; it was needed, he argues, to regulate the three-field rotation and enforce respect for the fallow land. An analysis of such documents as the *franches verités* in Hainaut suggests that the lords in fact shared their power with the more powerful and influent peasant clans (*coqs de village*).

Sivéry's ideas are stimulating, and he is surely right in preferring an attentive analysis *terroir par terroir* to dogmatic preconceptions. The title is misleading, however, as the author's brief excursions into regions beyond his primary focus, such as Catalonia or Touraine or the Alps, are made only to embellish his points. There are some minor errors in the text, and the absence of an index is annoying. Specialists of the questions treated may not find much that is new in this book; the author's attempt to oppose a *civilisation d'éleveur* to a *civilisation de cultivateur* seems to force the evidence. But many readers interested in medieval and particularly agrarian history will welcome Sivéry's shift in focus from manors to rural communities. His emphasis on medieval peasant initiatives is welcome when one considers the vigorous and independent peasantry of modern Western European history.

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MICHAEL E. GOODICH. *From Birth to Old Age: The Human Life Cycle in Medieval Thought, 1250–1350*. Lanham, Md.: University Press of America. 1989. Pp. x, 215. $29.50.

The human life cycle and its component stages have acquired the status of respectable subjects for investigation by historians. With this book Michael E. Goodich has made a useful addition to this body of literature from a perspective firmly planted within the field of medieval intellectual history. His focus is largely on the late thirteenth and early fourteenth centuries. While a long-established classical theory of

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the ages of man had noteworthy currency in the early and high medieval world, Goodich argues that after 1200 a growing appreciation of the physiological, psychological, and spiritual characteristics of each stage of life was displayed in a variety of texts.

Considerable attention is given to hagiographical and autobiographical sources with which the author is evidently very familiar, having used them in his previous work on medieval childhood. Their status is problematic, however, as Goodich admits, since in their role as exemplars they reflect an idealized account of behavior over the life course. He places greater weight on oratorical texts and encyclopedias to assemble a composite account of contemporary thinking on the "life cycle."

Goodich argues that these texts were reshaped in the twelfth century and expanded in the following century through the translation into Latin of Greek and Arabic science and philosophy. Outstanding among the new texts were parts of the Latin Aristotle; both his scientific and moral works were highly significant. The Aristotelian influence can be detected in the distinctly novel and secularized moral treatises, often composed in the vernacular, stressing appropriate patterns of child rearing. Another influence of signal importance was Avicenna's *Liber canonis*, appearing in university curricula for the first time in the thirteenth century. It provided detailed specification of the humoral transformations accompanying passage through life-cycle stages. Goodich carefully synthesizes the output of a large number of authors and purports to show how moral, pedagogical, and theological principles came to be derived from natural phenomena, thereby linking the ages of man to the unifying laws that were believed to govern the universe. For example, much is made of the poetic version of the natural philosophers' approach found in Dante's *Convivio*, which Goodich finds to be consistent with Albertus Magnus's commentary on the Aristotelian *Parva naturalia*: the four complexions and humors were regarded as the constituent elements, which effected change through the life-cycle stages, and the four stages paralleled the four seasons which mirrored the four parts of the day, and so on.

Goodich, to his credit, wishes to ground these intellectual changes within concrete developments in the real world. He singles out what he believes was a sharp rise in population from the late eleventh through to the late thirteenth century and regards it as the proximate cause of an intensifying interest in the human life cycle. Increasing numbers of young persons required child care and training and larger numbers of persons survived into old age, which necessitated a growing desire to extend life further. In this attempt to relate particular features of medieval thought to concrete and practical issues, Goodich's book makes some claim to be set alongside the work of historians such as John Baldwin, Brian Tierney, and Peter Biller, who acknowledge that medieval intellectuals were not only interested in abstract thinking.

Unfortunately, Goodich shows some uncertainties in his handling of the social and economic context. He accepts too readily some highly contentious arguments relating to demographic developments and changes in family, kinship, and marriage patterns after about 1100. Furthermore, few efforts are made to contextualize the evidence at the core of this study so as to alert the reader to its social, sexual, and, in particular, regional and geographical biases. A study of the material so valuably assembled by Goodich, paying greater heed to these biases, would build on this important pioneering "stage" in the study of the medieval life cycle.

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MALCOLM VALE. *The Angevin Legacy and the Hundred Years War 1250–1340*. Cambridge, Mass.: Basil Blackwell. 1990. Pp. xi, 317.

This book is at once bigger and smaller than the title suggests. Its central theme is the history of Aquitaine between the treaty of Paris and the outbreak of the Hundred Years' War. Malcolm Vale differs from most of his predecessors by directing the reader away from the formal processes of diplomacy and toward the personalities, political issues, and socioeconomic structures that conditioned Plantagenet, Capetian, and Valois claims to suzerainty and sovereignty in southwestern France.

Vale provides a vivid picture of the Gascon nobility and their political motivations, emphasizing the lawlessness and violence of society, the tendency toward feuding, and the resulting importance of effective arbitration. This provides a context for the better-known process of appeal to the *parlement* of Paris and the jurisdictional disputes that resulted between the duke of Aquitaine and the king of France. It also explains the motivations and loyalties of the powerful noble houses of Foix, Armagnac, and Albret.

Edward I was remarkably successful in maintaining loyalties, the more so since his quarrel with Philip IV and the resulting Capetian occupation of Aquitaine brought real hardship to the resident population. Where Edward I succeeded, Edward II failed. Indeed, the quarrel between Amanieu d'Albret and the seneschal of Gascony in 1311–12 threatened to escalate into a full-scale Anglo-French war. It was left to Edward III to win back his Gascon subjects and thus to procure the military forces needed to defend the duchy from the vigorous attacks of the Valois monarchy.

So far, all is well. Where this book disappoints slightly is in its failure to set the history of Aquitaine in a broader context. Aquitaine was only a part—albeit an important part—of the Angevin legacy, and

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the attitudes of King John, Henry III, and Edward I to the loss of their other Continental possessions certainly do not suggest that these or later kings gave up hopes of rebuilding the wider empire over which their Norman and Angevin predecessors had ruled. Vale himself stresses the "Frenchness" of the "English" monarchy throughout the thirteenth and fourteenth centuries in his opening chapter (pp. 9–47). In some ways this is the most exciting section of the book and it will certainly be read to great effect by students. Those readers, however, will also find it disconcerting that the book does not explore the fortunes of the other lost elements of the Angevin empire between the reigns of King John and Edward III. Vale comments that the early stages of the Hundred Years' War were marked by a deliberate reorientation of English ambitions away from the southwest and toward northern and western France. Had he placed Aquitaine within a larger geographical and chronological context, then the reader's perception of what actually comprised the Angevin legacy might have been rather different.

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N. J. Pounds. *The Medieval Castle in England and Wales: A Social and Political History*. New York: Cambridge University Press. 1990. Pp. xvii, 357. $39.95.

This book, N. J. Pounds tells us, is the result of more than fifty years of research, a statement easy to believe, for it is a work of immense scholarship in the grand positivist style of the early part of this century. Not only has the author mined the calendars of royal documents and seemingly most if not all the local antiquarian and archaeological journals of England and Wales but he has also, it would seem, visited, sketched, and mapped every one of the hundreds of sites he mentions, from the great Edwardian castles of north Wales to the most modest Midland mottes of the Conquest. And there seems to be hardly a fact or an observation he has noted in the field or in the library that has not found a place in these densely packed pages.

The table of contents gives scant account of the information the individual chapters in fact contain. The chapter on the Welsh and Scottish frontiers brings us *inter alia* a thumbnail history of the politics of these regions. The chapter on public administration brings us a brief account of the powers of local officials and the development of local administration. Along the way we sail on a sea of information and anecdotes, encountering here the hermit, elephant, and lion who lived cheek by jowl in the Tower of London, elsewhere some workers recycling the thatch and shingles of the houses in a bailey or accompanying lead roofing as it was carted halfway across England, yet elsewhere accounts that reveal how much it cost to garrison a castle in the thirteenth century. Along the way as well we watch as numerous received ideas are shattered: the cost of construction in stone put castle-building beyond the means of all but the wealthiest barons, in the twelfth century as in the fourteenth; only in exceptional cases, mainly on the frontiers, did castle baileys serve as refuges for surrounding populations; the advent of gunpowder at the end of the fourteenth century did not bring about a fundamental change in castle construction; and many others.

The overall theme of the book is simply stated. The history of castles began with ringworks and mottes, with wooden towers and palisades built to protect conquering lords in a hostile country or to overawe the populations of towns. It ended with crudely built stone towers—"peles" and "bastles"—that offered modest though necessary protection to late-medieval manorial lords, especially in the dangerous north. In between lay a short period of rapid building, when military campaigns meant siege warfare and small bands of armed men needed castles to protect themselves, followed by a long period of decay, as castles served more as administrative than as military centers, moats and baileys were invaded by houses, roofs decayed, and walls disintegrated. The castle finally became a sign of status, and royal licenses to "crenellate" a form of sumptuary control. But this is not a book to read for a thesis. It is one to dip into as a vast reservoir of information. It is an admirable capstone to Pound's distinguished scholarly career.

Fredric L. Cheyette
Amherst College

Jeffrey Richards. *Sex, Dissidence and Damnation: Minority Groups in the Middle Ages*. New York: Routledge. 1991. Pp. xii, 179. $29.95.

Over the past twenty-five years, an enormous body of excellent scholarly work has appeared dealing with minority groups in medieval Europe. The intellectual, social, and political processes by which the categories "heretic," "Jew," "gay person," "prostitute," "leper," and "witch" were constructed and employed to exclude such persons from the self-defining "mainstream" of medieval society are among the hottest topics of contemporary argument among medievalists. Controversy will no doubt continue, but a consensus is emerging on two points: first, that official attitudes and policies toward all these groups became sharply and simultaneously more hostile between 1150 and 1300; and second, that these groups were increasingly identified with one another. Witches came to be envisioned in the demonic terms already established for heretics and Jews; heretics, prostitutes, lepers, and Jews were forced to dress in distinguishing but similar costumes; like heretics, gay people were burned at the stake; and lepers and Jews were jointly accused of conspiring to poison wells across Christendom. The rise of intolerance in me-

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dieval Europe appears to be a chronologically and conceptually unified phenomenon. The challenge now is to explain how and why this extraordinary change in European social attitudes occurred when it did.

This is the challenge Jeffrey Richards has tried but failed to meet with this book. He makes no claim to offer the results of any original research. This is "an avowed work of synthesis" (p. xi) that relies entirely on secondary works. Even so, the book could have made a real contribution as a responsible, reliable summary of recent research on these different minority groups. Alas, that is not what we are offered. Richards opens with a chapter on "The Medieval Context," stuffed so full of hoary clichés, misinformation, and misunderstanding as to make one weep with frustration. To the extent the book has a unifying thesis, we are offered it in this chapter. Medieval intolerance was "the product of a puritan reformation following the millenium" (p. 13). Sexual nonconformity was the charge that linked these groups and stigmatized them all as social pariahs, according to Richards, although we are never told why he believes this charge to be so much more important than many others that were also used to justify the common persecution of these groups. The book has no conclusion, and from the number of phrases repeated word-for-word in different chapters, appears not even to have had the benefit of an attentive copy editor. None of the chapters rests on an adequate knowledge of even the essential secondary works. No one, for example, can write usefully about attitudes toward Jews in the Middle Ages unless they know the works of Gavin Langmuir, David Berger, William Jordan, and Kenneth Stow. All are absent from Richards's bibliography. Readers may well pick up some interesting material from the individual chapters on heretics, witches, Jews, prostitutes, lepers, and "homosexuals" (a term by no means transparent, but never defined). The absence of footnotes, however, will make tracing the source of this material frustrating in the extreme. Scholars and students alike will be far better served by Robert Moore's *Formation of a Persecuting Society* (1987).

ROBERT C. STACEY
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CARLO GINZBURG. *Ecstasies: Deciphering the Witches' Sabbath*. Translated by RAYMOND ROSENTHAL. New York: Pantheon. 1991. Pp. 339. $25.00.

Along with other historians of witchcraft (most notably Gábor Klaniczay and Gustav Henningsen), Carlo Ginzburg argues persuasively that vestiges of archaic shamanistic belief persisted into late-medieval and early modern European culture. He made this case earlier with respect to the *benandanti* of Friuli, and now he is extending the argument considerably. He does not suggest (à la Margaret Murray) that the accused witches were actually engaging in the ritual behavior they described. Rather, he argues that in trance states they fantasized about such behavior and that the content of their fantasies can be traced back to early stages of Eurasian culture.

Surely vestiges of shamanism did indeed linger in some places. Yet Ginzburg's discussion of vestigial shamanism as *explanandum* (accounted for in terms of primordial folk culture) and as *explanans* (accounting for the Sabbath myth) remains problematic. He is most concerned with the first of these problems, and devotes the bulk of his book to the etiology of the surviving shamanistic beliefs. His search for origins leads the reader on a breathtaking excursion through Celtic, Greek, Scythian, and other traditions, with side trips to ancient China and elsewhere. These sections of the book have something of an engagingly Ecovian flavor: one can almost hear Casaubon and Belbo piecing together the "plot," with connections seeming now plausible, now tenuous. Yet no one can accuse Ginzburg of dissembling, since he clearly labels conjecture as conjecture.

He is much less clear and thorough than one might like in unfolding the role of vestigial shamanism as *explanans*. Clearly he believes he has found the main popular source of beliefs in the Sabbath. For him, the inquisitors' diabolical Sabbath was essentially a learned overlay that concealed a popular shamanistic notion of the Sabbath. What happened in the case of the *benandanti* becomes the model for understanding the witch trials generally. Ginzburg seems to find this conclusion so self-evident that it scarcely requires proof. But the role of vestigial shamanism as *explanans* is precisely what Ginzburg has not established. Just when he touches fleetingly on these matters, and the reader expects evidence, Ginzburg instead heads off into a disquisition on Cinderella and her relationship to Oedipus.

A small number of trials seem fairly clearly to rest on such foundations: I pointed out some years ago (without cognizance of the "embarrassment" that Ginzburg projects onto me) that neither typical sorcery nor conventional diabolism figures in the Milanese trials of 1384–90, and Ginzburg's interpretation of these cases is convincing. In most of the other cases he cites the evidence is more slender. (Does being a sorcerer or "attending," as the phrase goes, "the good society," really show that the wealthy physician Baldassarre Pestalozzi was in fact acting as a shaman?) Even when shamanistic notions, or their parallels or vestiges, do occur, it is by no means clear what role they played within each specific cultural matrix or in the provocation of the trials. Folk traditions about night-flight and animal metamorphosis might be associated with specific living shaman figures (like the *benandanti*), or they might survive only as vague memories, revived and applied only incidentally to specific persons already brought to court on other charges. Ginzburg himself cites the analogous case of Fraticelli accused, among other things, of

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associating with "the Sibilla and the Fairies," but he does not pursue its implications. Once a trial began, every manner of accusation and confession could arise. Ginzburg does not show how to distinguish among the various possibilities: how to discern whether vestigial belief in shamanism played a crucial or only an incidental role in a specific trial, when such notions arose at all.

When Ginzburg writes about the actual trials, he remains unclear about how far he would want to extend his conclusions. Would he want us to assume that Johannes Junius of Bamberg was (or was seen as) a shaman? That the accused at Salem were bearers of archaic shamanistic tradition? What Klaniczay has said in connection with the Hungarian trials applies elsewhere: cases of this sort appear to have constituted a small minority of the witch trials. When we have clear evidence of how a series of trials began, in the great majority of cases the initial concern was with *maleficia*.

Ginzburg eschews the term "diabolism," although it is clearly appropriate for the theological overlay imposed on popular notions (regarding sorcery, shamanism, or whatever else). Why he should be sensitive on this point is unclear, since he recognizes that this overlay was something distinct from the popular Sabbath he is trying to re-create. The question is how the two phenomena, popular and learned, related to each other. The learned notion of diabolism may in some ways have paralleled the shamanistic concepts Ginzburg investigates, and may in some measure have been influenced by judicial experience with these notions. Yet when all is said and done, the shamanistic "Sabbath" resembles the demonologists' Sabbath strikingly little, in spirit and even in detail. Learned diabolism grew mostly from other roots, for which Norman Cohn remains our best source.

RICHARD KIECKHEFER
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GAVIN I. LANGMUIR. *Toward a Definition of Antisemitism*. Berkeley and Los Angeles: University of California Press, with the cooperation of the Center for Medieval and Renaissance Studies, University of California, Los Angeles. 1990. Pp. x, 417. $45.00.

As a graduate student two decades ago, while writing my first serious seminar paper on the Jews of Normandy under Saint Louis, I came across an article published by Gavin I. Langmuir in 1960. (It is included in the collection under review and is, indeed, the oldest article in the collection.) That article, with the provocative title, "'Judei nostri' and the Beginning of Capetian Legislation," was an eye-opener, showing that it was possible to do the most rigorous kind of administrative history and combine it with fascinating, thoughtful social history.

In the article Langmuir argued that the first real legislation in what we call France (at least, the first since the Carolingians) concerned Jews, and he raised the possibility that there was nothing accidental about this. Barons could be induced or forced to accept the mandates of the crown on matters related to Jews for a whole host of reasons; once it was established that these barons could not go their own way on the issue of Jewish policy, it became considerably easier for the crown to make powerful claims about its legislative authority in many other areas. The views, arguments, data, and hints at new interpretations in that article were so seductive that I regarded it then as a classic. I still do.

There are several other classic pieces in this collection. Langmuir's study of the analogy of serfdom as applied to Jews in the Middle Ages, "'Tanquam Servi': The Change in Jewish Status in French Law about 1200," is a masterly review of this misunderstood topic. It takes as its starting point the unfortunate tendency of earlier historians to generalize about Jewish status across the map of medieval Europe on the basis of one or two famous statements. Langmuir shows that a great deal of diversity existed and, most important, that the law of status itself had a history. Humbly, he even corrects his own views.

Three other articles deserve special mention. Two are splendid exercises in the historian's craft. Langmuir's reconstruction of how the myth of ritual murder was first constructed by Thomas of Monmouth in Norwich in the aftermath of the death of a child in 1144 shows the historian as detective at his best. And Langmuir's magisterial exploration of the evidence on the tale of Young Hugh of Lincoln, another alleged child victim of Jewish perfidy, is a blueprint for how to squeeze sources for everything they are worth in order to get at "the truth." Assigning it to undergraduates, as I once did, had its problems; it is a difficult piece of work. But the best students came away from the encounter with a deep appreciation of how difficult it is to do medieval history honestly and how stupid simple-minded positivism really is.

The third of these three articles is here called "The Transformation of Anti-Judaism" (its original title is much more accurate: "From Ambrose of Milan to Emicho of Leiningen: The Transformation of Hostility against Jews in Northern Christendom"). It is an evocative, occasionally heated piece that touches on a whole host of issues. One of its most accomplished *tours de force* is the almost clinical excision from received wisdom of the traditional notion that the so-called popular expedition of the First Crusade was aberrant. Langmuir brings to light the hidden ideological agendas of many of the scholars who have insisted on this rather arbitrary assessment, by which violent anti-Jewish actions associated with the popular expedition are typed as atavistic, vulgar, and the result of poverty and wretched or misguided education, while people who professed "true" Christianity were free of the excesses of the mob. But, Langmuir insists, the argument fails for many reasons, not least

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because Emicho of Leiningen, one of the leaders of the popular crusade, was an elite member of society, a knight, who deeply professed traditional Christianity.

Why has the appropriate title of the article just discussed been changed for the collection? I think the reason is obvious, and it may explain as well the title of the collection as a whole, *Toward a Definition of Antisemitism*. Langmuir's own intellectual biography has propelled him to try to probe the very roots of anti-Jewish sentiments in Western culture, and increasingly he has employed diverse tools from other disciplines—sociology, psychology, philosophy, anthropology—to do so. Recently he published a ruminative discourse on religious thought and prejudice under the title *History, Religion, and Antisemitism* (1990), to which he refers in the introduction of this collection. Moreover, the newer work (some hitherto unpublished articles) in this collection also enlarges on the themes of the origins, persistence, pervasiveness, and structural characteristics of anti-Jewish sentiments.

No one can deny the importance of these subjects. No one should criticize the effort of a distinguished scholar to probe them. But the results do not seem to me to be of the same seductiveness or persuasiveness as the more narrowly historical work that Langmuir has done. I come away from the theoretical excursus unenlightened, feeling that they are somewhat overwritten and overheated bibliographical essays in which commonplaces are elevated inappropriately to the level of great human truths. I certainly do not believe that I understand violent anti-Jewish behavior better because of these theoretical musings.

As a historian, Langmuir has provided other scholars with some of the most arresting and vivid reconstructions of the past that they have read. He has done so with sensitivity and compassion. His effort to expand his horizon—to go from Capetian legislation, the tale of little Hugh of Lincoln, and the viciousness of Emicho of Leiningen to a "definition of antisemitism"—is unsuccessful to my mind, but no less courageous and admirable for that.

William Chester Jordan
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MODERN EUROPE

Maurice Cranston. *The Noble Savage: Jean-Jacques Rousseau, 1754–1762*. Chicago: University of Chicago Press. 1991. Pp. xiv, 399. $32.50.

This book is the second of a projected three-volume biography of Jean-Jacques Rousseau by the British historian of political thought, Maurice Cranston. *Jean-Jacques: The Early Life and Works of Jean-Jacques Rousseau* (1983) covered the first forty years of Rousseau's life, and the final volume will follow him through the last fifteen. The present work handles only 1754–62, but these were the most significant years, both in terms of Rousseau's productivity and because it was then that he broke with the philosophes. In this period he wrote and published the *Lettre à d'Alembert*, *La Nouvelle Héloïse*, *Émile*, and *Du Contrat social*. Cranston discusses these texts intelligently, including their complex publishing history, without losing the thread of his narrative.

Rousseau himself covered all this ground in books nine through eleven of his *Confessions*. Cranston's division shifts the emphasis away from the early years treated by the first eight books of the *Confessions* and toward the brief period when Rousseau wrote his major works. While Rousseau gave us "the portrait of a man," Cranston is giving us the life of a writer. The great strength of Cranston's work lies precisely in the degree to which he has been able to wrest his story from the dominating portrait that Rousseau left behind.

To this end, Cranston employs what he calls a Lockean epistemology (p. ix) to examine a wealth of sources beyond the *Confessions*. He takes particular advantage of R. A. Leigh's edition of Rousseau's correspondence (1965–91) in order to balance the retrospective views his subject expressed in his autobiography. The result is an admirably steady and sane biography of a man who was not noted for being either. While Rousseau's voice comes through loud and clear in substantial, well-translated quotations, Cranston's measured tone always guides us. This volume is both well written and a good read.

Telling the story of Rousseau's life, however, requires balancing his perspectives against those of the people with whom his life was entangled. And here is where Cranston's objectivity fails him. While he maintains a scrupulous neutrality toward Rousseau by declining to judge him, he is not nearly as generous when it comes to others. For example, in trying to sort out the truth of one of the many personal conflicts that defined this period of Rousseau's life, Cranston writes that Rousseau's mistress, Thérèse, told one version of the story and Mme. d'Epinay, his friend, another. "The likelihood," he concludes, "is that neither Mme d'Epinay nor Thérèse told the truth . . . Rousseau himself seems to have been perversely blind to the possibility of Thérèse being just as jealous as Mme d'Epinay" (p. 67). There are two points to be made here: first, Cranston suggests that Thérèse and Mme. d'Epinay lied, while Rousseau was merely "perversely blind"; and second, he does not seem to notice that the only source he has for Thérèse's "lie" is Rousseau's report of it in his autobiography. This uncritical use of the *Confessions* can be compared to the highly critical way in which Cranston refers to Mme. d'Epinay's *Histoire de Madame de Montbrillant*, a *roman à clef* based on the same material as Rousseau's autobiography, but with a decidedly different perspective.

While his friends all deceive Rousseau at one point or another in Cranston's narrative, the "Noble Savage" never lies. Cranston repeatedly reminds us that

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he believes Rousseau, although what he tends to believe is that Rousseau really felt what he says he did. How Rousseau felt, however, is hardly at issue. To privilege sincerity is to succumb to a Rousseauian epistemology, despite the richness of detail and the wealth of evidence that gives the book its Lockean pedigree. I suspect that until Mme. d'Epinay is taken as seriously as Rousseau, an "objective" biography will not be written.

It takes courage to retell the story that Rousseau told so seductively in the *Confessions*, and Cranston should be congratulated for rising to the challenge and for writing a book that is both impressively researched and a pleasure to read. For Rousseau's many partisans, this should be enough. For those of us whose sympathies lie elsewhere, it is not quite as satisfying.

DENA GOODMAN
Louisiana State University

DENA GOODMAN. *Criticism in Action: Enlightenment Experiments in Political Writing*. Ithaca: Cornell University Press. 1989. Pp. vii, 244. $29.95.

In this study, Dena Goodman brings new perspectives and analytic methods to bear on a principal issue in Enlightenment studies: the connection between critical thought and action and, in its most general formulation, between Enlightenment and revolution. Drawing on recent work in literary theory, including studies of form, structure, and the performative uses of language, Goodman interprets Baron de Montesquieu's *Persian Letters*, Jean-Jacques Rousseau's *Second Discourse*, and Denis Diderot's *Supplement to the Voyage of Bougainville* as critical texts directed toward an implied reader (no longer the prince, but the public) and structured to shape that reader's critical powers.

Goodman offers a brilliant, insightful discussion of how (and within what limits) epistolary, narrative, and dialogic forms shape and develop the critical activity of the implied reader. For example, Goodman argues that form and textual strategies in the *Persian Letters* work to involve the reader in integrating the parts of the text into a coherent whole, engage his reason and common sense in comparative cultural analysis, and draw him into recognizing and applying universal concepts and values like justice and toleration; in short, the text operates to develop and sharpen the reader's critical faculties. The final letters, however, depict the tragic failure of Usbek, the critical thinker in exile, to identify and reform in his own household the despotic abuses of authority he exposed and condemned in comprehensive criticism. The message to the implied reader appears to be that criticism is a dead-ended act.

While Montesquieu in the *Persian Letters* and also Rousseau in the *Second Discourse* leave the implied reader with no possibility of invoking criticism as an instrument of change, Diderot, in the *Supplement*, structures a role for the reader as critic-reformer. In the text, B guides A through a reading of a supplement to Bougainville's voyage. As the two readers engage in comparative cultural analysis and criticism, they identify principles of universal natural law and instances of their operation in Tahitian society. They explore possibilities of reforming oppressive positive laws in France to bring them into closer conformity with natural law and particular individual and collective needs. At the end of the dialogue, B adopts a critical attitude, one which the implied reader of the dialogue is situated to appropriate and implement: the individual's obligation to persist in criticizing unjust, arbitrary laws until they are reformed is as compelling as the duty to obey them; critical activity is inextricably bound up with the political commitment to reform. In Goodman's phrasing, "reform is simply criticism in action . . . The critic is now seen to be the agent of political change through reform. And the critic is not simply Diderot, the author of the *Supplément* . . . but the reader of that book as well" (p. 225).

One of the principal merits of Goodman's book is that it enriches and deepens our understanding of the potential political power of critical literature as an agency of change in Enlightenment culture. Did Enlightenment criticism in fact have that impact? Such questions take us outside the boundaries of Goodman's study (with its principal focus on the formal analysis of how texts structure the critical acts of an implied reader) and into closely historicized, contextualized investigations of the writers behind the texts, their actual readers, what the former intended to convey, how the latter read the texts, and where the bridge of Enlightenment criticism in fact took those who spanned and crossed it. In the field of Enlightenment intellectual and sociocultural history, there is a place and need for both perspectives on the relation between critical thought and political acts.

DARLINE GAY LEVY
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PETER BROCK. *The Quaker Peace Testimony, 1660–1914*. York, England: Sessions Book Trust; distributed by Syracuse University Press. 1990. Pp. viii, 387. Cloth $40.00, paper $19.95.

In a revision of his own work on American Quaker pacifism, Peter Brock has given the scholarly community the first general treatment of Quaker pacifism before World War I since Margaret Hirst's *The Quakers in Peace and War: An Account of Their Peace Principles and Pacifism* (1914). Brock has written a good, detailed account and draws on many studies that have been published since his earlier books appeared two decades ago. In this regard, where he and others have done the basic research, Brock's work is full; where they have not, there remain gaps, especially for the nineteenth and twentieth centuries.

Approximately half of Brock's account of pacifism

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details Quaker pacifism from its origins through the American Revolution, and most of that discussion focuses on the American colonies. He has adjusted the examination of English Quaker pacifist origins to take in the work of Barry Reay and Christopher Hill. The chapters that follow on Friends in the colonies range from discussion of conscientious objection to war to Quaker participation in governments that were caught up in military activity. Brock has accurately used the basic research of others on Friends' activities in this period. He has also carefully dealt with disagreements among scholars while using their work to fill out his account.

For the period after the American Revolution, Brock has been less well served by the secondary works, and for that reason his treatment of this period is frequently episodic. Missing, for example, is a discussion of Quaker reactions to Indian wars in the United States in the nineteenth century. Surely, Quaker humanitarian efforts for Native Americans, however uncertain and wrongheaded as they may have been in that period, had a counterpart in criticisms of these wars. His account broadens after the American Revolution to include a chapter on the Shaker peace testimony (at best a marginal choice for, whatever the origins of Ann Lee, she and her followers were not Friends), followed by individual chapters on Canadian, French, Norwegian, and Prussian Quaker pacifism. The result is an unfortunate exaggeration of the importance of areas where Friends were few. After scholars do more work on Quakers in the late nineteenth century, Brock and others will be better served. Meantime, he has worked well with available studies for the nineteenth and twentieth centuries and, where he has had secondary works on England and the United States in that period, his conclusions are sound.

Many readers will enjoy the extensive editorial comments in the notes. Brock is at his avuncular best in accepting and rejecting others' conclusions. The notes also provide something of a bibliographic essay, albeit a fragmentary one, and one frequently delivered in the first person. Occasionally, there is repetition in citations, and the index should have been expanded to include many of the subjects with which Brock deals forcefully in text and notes. Despite these criticisms, Brock's work remains a major contribution to pacifist studies.

ARTHUR J. WORRALL
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HELENA WADDY LEPOVITZ. *Images of Faith: Expressionism, Catholic Folk Art, and the Industrial Revolution.* Athens: University of Georgia Press. 1991. Pp. xviii, 228. $40.00.

When Vassily Kandinsky and Gabriele Münster left Munich for summer vacations in the Bavarian countryside around the turn of the century, they were intrigued by the religious folk artists they found there and the paintings on glass these artists executed. The sophisticated urban artists saw these works as part of an age-old folk tradition, one that they quickly appropriated, hoping, in the expressionist mode, to bring a popular vitality into academicized studio art. Helena Waddy Lepovitz's study of painting and painters on glass in southern Bavaria tells quite a different story, pointing out the recent vintage of such "traditions" and examining their origin, rise, growth, and decline in the economic context of the Alpine peasantry and the cultural context of Bavarian Catholic devotion.

The author convincingly describes glass painting as an unusual version of protoindustrialization, the system of marketing on a broad scale the products of artisanal, rural outworkers who flourished throughout Europe during the eighteenth and much of the nineteenth centuries. Glass painting began in several resource-poor villages of southern Bavaria in the 1770s as a way for peasants to survive on their increasingly subdivided family plots. The influence of Augsburg, a major center of craft production, and of Munich, already an important art market, helps explain how this particular uncommon craft found its way into the countryside. The first half of the nineteenth century was a boom time for the business, and glass painters intensified their productive capacity, even introducing a division of labor. Such production could reach quite impressive levels; Lepovitz recounts how, in 1816, Matthias Noder of Seehausen contracted to produce 4,000 religious paintings in two months for sale at a church jubilee.

As with other forms of outworking, industrialization was the downfall of this one. The introduction and perfection of chromolithography in the two decades after 1850 provided a cheaper way of producing popular devotional pictures; low-cost mechanized competition gradually drove the painters on glass out of business. Their prior experience of working for the market, however, enabled the former glass painters to adapt to changing economic conditions by turning their villages into tourist centers and attracting vacationers from Munich, including the expressionist artists, thus bringing the author's story full circle.

An intriguing story, clearly and concisely written, elegantly and excellently produced, all combine to make a book thoroughly worth reading. At times, however, the author strikes a false note concerning the broader context of her work. While her discussion of the religious and cultural universe of the painters and of the consumers of their goods includes abstract sociological polemics and the introduction of questionably relevant examples, such as the Marian devotion of the Indians of southern Mexico, it neglects significant studies of Bavarian popular and religious culture, particularly the works of Werner Blessing (for example, *Staat und Kirche in der Gesellschaft: Institutionelle Autorität und mentaler Wandel in Bayern während des 19. Jahrhunderts* [1982]). Glass painting as

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a form of protoindustry is never compared with more common kinds of outworking, such as textiles or metallurgy. Lepovitz also does not explain how, if the marketing experiences of outworking enabled former glass painters' villages to become tourist centers, many other, more agriculturally oriented villages managed the same transition. These omissions lessen the broader significance of an otherwise fascinating story.

JONATHAN SPERBER
University of Missouri, Columbia

JOSEPH S. FRUTON. *Contrasts in Scientific Style: Research Groups in the Chemical and Biochemical Sciences.* (Memoirs of the American Philosophical Society, number 191.) Independence Square, Pa.: American Philosophical Society. 1990. Pp. ix, 473. $40.00.

A distinguished biochemist turned historian of science, Joseph S. Fruton here pursues his longstanding interests in the historical relations of chemistry and biology, focusing on the appearance and functioning of large research groups chiefly in nineteenth-century German chemistry and biochemistry. Believing that the dominant formative influence on these groups is the style of leadership exercised by the individual scientists who founded them, and that a largely overlooked function of the groups is the production of the next generation of practitioners, Fruton attempts to assess the "distinctive modes of thought and action—the scientific styles—that have characterized the leadership of research groups in the chemical and medical sciences" (p. 2) and to exhibit the extent to which the leaders' characteristics (whether as visionaries, practitioners, or group leaders) are perpetuated in the work of their scientific progeny. Fruton examines the groups of several highly prominent chemists and biochemists of the nineteenth century (Justus Liebig, Adolph Baeyer, Emil Fischer, Felix Hoppe-Seyler, Franz Hofmeister, and Willy Kühne), explores their styles of leadership, collects and synthesizes much data on the composition of their groups and the fate of their members, characterizes their research and its outcomes, and attempts to offer some broad generalizations about the impact of leadership on the development of chemistry and biochemistry.

He identifies two diametrically opposed leadership styles, dictatorship and liberality. In the former an autocrat runs a closely directed group, takes most of the credit for its achievements in research (considered to result from his formulating and guiding the investigations of the group), and expects junior associates to derive satisfaction from the leader's reflected glory, leaving them with the hope that he will make their accomplishments known to potential employers. The liberal director, by contrast, serves as an unselfish senior advisor to junior associates, takes little explicit credit for their achievement (despite perhaps having suggested and profoundly shaped their investigations), and bases his own reputation on his personal research, which likely dates mostly from early in his career. Fruton seems also to associate with liberality a greater breadth of vision for the disciple and greater success in seeing the leader's views realized in the accomplishments of the next generation, a perspective that he believes to offer an escape from the customary hagiography characterizing many studies of prominent scientists and a corrective to the failure of historians to assess the significance of the less-prominent scientists. Briefly taking up more recent research groups in a final chapter, Fruton detects the persistence of the contrasting patterns, although he finds the closely directed group the dominant, while upholding his conviction that liberality is more able to mold the future course of research. Thus, in this perspective, "Hoppe-Seyler, Hofmeister or [F. Gowland] Hopkins were . . . more effective than . . . their equally distinguished contemporaries Kühne and [Albrecht] Kossel" (p. 275).

Fruton writes mindful of the suspicion with which scientists-turned-historians are viewed by professional historians of science (pp. 12–15), and his wariness is unfortunately justified. Although his book aims to study the relations of leaders to junior associates, thus constituting a chapter in the history of "social relations of science," Fruton asserts the primacy of "internal history," that is, the history of scientific ideas. He associates himself with a small but significant literature aimed at analyzing the emergence of modern forms for the organization of research; but he accords a central theme of that literature, the significance of disciplinary and similar aggregative distinctions for informing the development of science, a treatment that, depending on one's perspective, is either cavalier or naive. He therefore imposes on his data his own conception of "the biochemical sciences" (pp. 4–5), dismissing contemporary claims about the structure and jurisdiction of disciplines as so many "manifestations of academic rivalry" (p. 5), and he uses his internalist tools as the foundation for identifying the significant groups, their significant members, their major achievements, and their leaders' success in affecting the future. Yet such an assessment, particularly of the fate of leaders' visions, can scarcely flout contemporaries' conceptions of their enterprise. Moreover, Fruton's own narrative frequently points to the powerful influence of institutional setting, university politics, and industrial development in molding the character and magnitude of research; acknowledged early on (p. 2) and frequently noticed in the text, they nevertheless are decisively overshadowed by the autocratic/liberal contrast and its associated internalist foundations.

Fruton unquestionably does a service by seeking a route out of traditional hagiography by presenting much data on the composition, structure, and functioning of large groups; but his study reveals the need

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for a balanced account. An approach that combined Fruton's strengths with the sensitivity to context exhibited by Steven Turner (in *Historical Studies in the Physical Sciences* 13 [1982]) or William Coleman (in F. L. Holmes and Coleman, *The Investigative Enterprise* [1988]) would better illuminate the structure of the research enterprise and its effects on the development of science.

Evan M. Melhado
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Lee Congdon. *Exile and Social Thought: Hungarian Intellectuals in Germany and Austria, 1919–1933*. Princeton: Princeton University Press. 1991. Pp. xvi, 376. $29.95.

The late Hungarian historian Zoltán Horváth identified the plethora of talented intellectuals of turn-of-the-century Hungary as the second reform generation. He observed that while the goals and achievements of the first reform generation were cut short with the 1848–49 revolution, the work of the second reform generation ended with World War I and the consequent collapse of the Austro-Hungarian monarchy, and with the failed revolutions of 1918–19 (*A Magyar századforduló: A második reformnemzedék története* [1974], 12–16).

In his excellent monograph, Lee Congdon deals with intellectuals, most of whom were identified by Horváth as belonging to the second reform generation. These men and women were forced to emigrate after 1919. Congdon goes further than Horváth, however, when he proposes and successfully demonstrates that they continued their work, mostly in Germany and Austria, and "it is only a slight exaggeration to say that exiled Hungarians *created* Weimar culture" (p. xi).

These émigrés made themselves known as masters of ideological trends such as Marxian, guild, and Christian socialism, and liberalism. Others, putting less emphasis on politics, came to be known for their contribution to sociology and to avant-garde art. Although their contributions were diverse, a common goal that motivated these intellectuals was a search and rediscovery of community (*Gemeinschaft*). The intellectuals whose works Congdon discusses in some detail include Georg Lukács, Béla Balázs, Lajos Kassák, László Moholy-Nagy, Oszkár Jászi, Ilona Duczynska, Karl Polányi, Aurel Kolnai, Paul Szende, and Karl Mannheim. Congdon, however, identifies and recognizes contributions of other brilliant but less well-known Hungarian émigrés.

Throughout the book Congdon demonstrates the validity of his thesis that these Hungarians were shapers of Weimar culture. Lukács's publication *History and Class Consciousness* (1923), while denounced in Moscow, had a lasting impact in Germany, where it was seen as articulating a new kind of Marxism. Through this work Lukács became the spiritual leader to the founding of the Frankfurt Institut für Sozialforschung. Another Marxist who joined the German Communist Party in 1931, Balázs made important contributions to German filmmaking as a screenwriter. More important, he became known as the leading theoretician of film (p. 115). More obscure Hungarian Marxists such as Andor Gábor and Lajos Barta had their plays produced in Germany. These plays "prepared the way for Bertold Brecht's 'Epic theater'" (p. 56). Among the avant-garde artists who contributed to Kassák's artistic magazine *Ma* was Moholy-Nagy, who in 1922 was appointed professor at the Bauhaus by Walter Gropius. Congdon sees Moholy-Nagy as a "commanding figure, who took the Bauhaus in a new direction" (p. 181). The Bauhaus provided positions and jobs to many other Hungarians, including the furniture maker Marcel Breuer.

Mannheim, a sociologist and philosopher, was appointed as *Privatdozent* at the University of Heidelberg in 1925. In 1929 he published his *Ideology and Utopia*, which was influenced by Lukács's and Szende's writings and by gestalt psychology. The book brought about a lively debate in some academic journals. This debate, which was joined by Hannah Arendt, Paul Tillich, and Herbert Marcuse, among others, is identified by Congdon as a major event in the history of modern social thought. As a result of the publication, and in recognition of Mannheim's scholarly contribution, he was appointed professor of sociology at the University of Frankfurt in 1930, which he had to vacate when the Nazis came to power.

In his brief conclusion, Congdon attributes the success of the émigré intellectuals, who were mostly assimilated Jews, to the fact that, being identified as "alien" by the official and unofficial propagandists of the Horthy era, "they attached themselves to larger families: international communism, the international avant-garde, the Bauhaus, the Austrian Social Democratic Party, the Roman Catholic Church, the free floating intelligentsia" (p. 304).

It is a sad commentary on the interwar Hungarian government that it willfully sacrificed some of Hungary's best thinkers to a brain drain that was nothing short of a cultural catastrophe.

Peter Pastor
Montclair State College

Pierre Sorlin. *European Cinemas, European Societies, 1939–1990*. (Studies in Film, Television, and the Media.) New York: Routledge. 1991. Pp. viii, 247. Cloth $59.95, paper $18.95.

Pierre Sorlin, the author of several respected sociological treatments of European cinema (including the excellent *Sociologie du cinéma* [1977] and *The Film in History* [1980]), analyzes the national cinemas of Great Britain, Italy, France, and Germany using the films

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produced there as insight into comparative European social history. In the light of the many discussions over a possible "pan-European culture" following the 1992 deadline for the abolition of internal European frontiers within the Common Market, Sorlin's treatment of European cinema as a unitary phenomenon is a welcome first step toward a comprehensive history of contemporary European cinematic culture.

Sorlin treats films as "images sold on the market" rather than completely original, unique works of art, and while this approach to the subject may well offend formalist critics, in Sorlin's capable hands the sociological approach yields a number of interesting insights into European cinema as an art form as well as a social phenomenon. Perhaps the most refreshing aspect of Sorlin's treatment of European cinema is that the author never neglects the fact that the context for European film has consistently been dominated by Hollywood and American popular culture rather than by European production companies or European popular culture. Precisely because Sorlin is so well versed in Hollywood cinema, he is able to underscore the ways in which European cinemas develop in directions different from those of their American counterpart.

Chapters are devoted to European images of the coming war, images of the Nazi resistance, the golden age of European cinema in the decade following the end of World War II when the European film market was at its peak and more than three million admission tickets were sold, and the image of cities and urban environments in the European cinema (including discussions of shanty towns and immigrants). Each of these chapters is organized along thematic lines and will have something to offer the contemporary historian or sociologist, as Sorlin's familiarity with the cinema of several nations is quite impressive. Two additional chapters discuss how European directors attempted to modify, sometimes with great success, the classic Hollywood narrative style. Sorlin's concluding chapter offers a number of fascinating generalizations about European cinematic culture that should stimulate readers from a variety of fields, including film studies, cultural history, and sociology.

No doubt many film critics and historians will express serious reservations over Sorlin's account of European cinema, primarily because they will wish to focus on the two chapters analyzing Europe's response to classic Hollywood narrative. In fact, these chapters are the least convincing in Sorlin's book, precisely because this sociological approach works far better when dealing with thematic issues (those in the first four chapters) rather than aesthetic problems such as narrative form. But any comprehensive history of European film must deal with both the sociology of the cinema and the aesthetics of its sociological themes—not only how social problems are reflected in the European cinema but also how such problems find concrete formalist expression in a work of art. Written in a readable style free of jargon and based on an impressive grasp of the bibliography in a number of languages and national cinemas, Sorlin's overview of European cinema and its relationship to European society during the past half-century will certainly represent the springboard for any future discussion of the topic.

PETER BONDANELLA
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MICHAEL J. SODARO. *Moscow, Germany, and the West from Khrushchev to Gorbachev.* (Studies of the Harriman Institute.) Ithaca: Cornell University Press. 1990. Pp. xiv, 423. Cloth $49.95, paper $15.95.

Just as the Berlin Wall suddenly appeared in August 1961, dramatically symbolizing inter-German division and the West's conflict with the Soviet Union, almost as precipitously in the winter of 1989 the wall came "tumbling down" and reunification followed. Ironically, the wall's destruction took place for precisely the same reason that it was initially constructed, to keep East Germans from flooding westward.

Although most observers of Soviet-German affairs would admit surprise at the speed and completeness of the change that has befallen the Germanies, Eastern Europe, and the Soviet Union, Michael J. Sodaro suggests the present condition is merely the reasonable evolution of past events and interrelationships. His careful examination of Soviet and German political structures and opinion provides evidence of continuity rather than discontinuity. While the Soviets clearly dictated policy to the German Democratic Republic (GDR) and set the tone for relations with the Federal Republic (FRG), internal division among Soviet policy makers not only provided opportunity for a high degree of East German independence but also made for a much more flexible Soviet policy vis-à-vis West Germany. Here is where Sodaro makes a significant contribution. He exposes the great diversity of opinion existing among Soviet and East German primary and secondary elites regarding Soviet-German and GDR-FRG relations. He separates policy makers into four schools of thought: maximalist, internationalist, Europeanist, and Americanist. These personnel categorizations, plus his analysis of Soviet and GDR secondary elites, will probably be most interesting to scholars because a conceptual framework is established, making possible more sophisticated studies of alternative attitudes toward the West. Nevertheless, Sodaro's skillful and thoughtful portrayal of the primary elites is a work of art: Nikita Khrushchev, complex, overbearing, truly wanting good relations with Bonn, an excellent model of a reformer influenced by older ideological assumptions; Leonid Brezhnev, the conservative technician who believed he could pursue economic cooperation with the Federal Republic while targeting additional NATO bases; Mikhail Gorbachev, the pragmatist who

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sometimes failed to understand the full magnitude of his actions; Walter Ulbricht, the premier ideologue who hoped to maintain GDR independence by making that country economically indispensable to the Soviet Union; and the more politically flexible Erich Honecker, who saw no contradiction or danger to East Germany's independence by promoting closer economic ties with West Germany or supporting arms limitation. Equally useful are Sodaro's comparisons of Willie Brandt, Helmut Schmidt, and Helmut Kohl, and his description of the East European impact on Soviet-German relationships. Especially intriguing is his view that the Czech crisis in 1968 actually gave the Soviets a sufficient feeling of security in Eastern Europe to accept accommodation with the Federal Republic as non-threatening. What irony that such a tragic moment for the Czechs could be considered the turning point marking the beginning of Ulbricht's demise, improvement of Soviet-FRG relations, and further movement toward arms control.

This volume is an excellent first assessment of Soviet-German relations from the late-Khrushchev era through 1989. Sodaro has done a fine job mobilizing an enormous variety of published sources. There are a few issues that one might have wished Sodaro would have considered more fully. For instance, there is only limited coverage of Soviet economic and ethnic problems as foreign policy determinants—in particular, the impact of the turmoil originating in the Baltic states. One might also have hoped for a fuller account of the impact of weak Soviet leadership under an ailing Brezhnev, Yuri Andropov, and Konstantin Chernenko. Moreover, Sodaro's characterization of Gorbachev may also be a bit too charitable. Certainly no one would deny his political adaptability or his contribution to arms reductions, but what about his association with Andropov, his original motives for reform, his inconsistent economic and nationality policy, and his willingness to accept conservative solutions? Finally, while the identification of specific "camps" among East German and Soviet elites appears to be a valuable contribution for the purpose of analysis, the frequent shifting of views among policy makers partially undermines the value of establishing categories in the first place. Despite these few concerns, this volume constitutes a giant effort that will be appreciated by scholars and students alike.

JACK DUKES
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GREG WALKER. *Plays of Persuasion: Drama and Politics at the Court of Henry VIII*. New York: Cambridge University Press. 1991. Pp. xii, 244. $49.50.

This is a study of the role of court drama during the reign of Henry VIII. In his analysis of the texts of five plays (*Hick Scorner*, John Skelton's *Magnyfycence*, *Godly Queene Hester*, John Heywood's *Play of the Weather*, and John Bale's *King Johan*), Greg Walker sets out to show how drama was designed to sway the minds of powerful figures, for it was not merely an amusing diversion but also an opportunity for persuasion and advancement. He establishes that most plays were written for performances in the environs of the court and for the social circle in which the king moved.

Since playwrights seem to have designed their works for performances at definite locations and for generally known audiences, a successful identification of their political intentions would be useful. Unfortunately, so little information is known about when and by whom the plays were composed or about where and even if they were produced that most of Walker's suggestions, however reasonable, must remain speculative. For example, noting that *Hick Scorner* was probably written for performance in Southwark, he concludes that it was likely meant for the entertainment of Charles, Duke of Suffolk, a local landowner. To support his observations here Walker relies heavily on textual analysis, for no hard evidence is extant concerning its actual production or composition.

He is on firmer ground with Bale's and Heywood's dramas. A play about King John was performed in 1538–39 before Thomas Cranmer, Archbishop of Canterbury, and Bale, who wrote a piece about that king, was then a client of Thomas Cromwell. Ultimately most playwrights may have hoped to persuade Henry VIII of their opinions, as Walker argues. But he might well also have considered that Bale's goals could have included using his work, which presented the papal schism as doctrinely motivated, to persuade Cromwell, who may have been less committed to radical reform than is usually claimed, to continue his public support for that religious position at a time when it was under increasing challenge.

Walker's most successful analysis is of Heywood's *Play of the Weather*. The drama of this playwright, a member of the royal household, was presented at court, although no explicit evidence about this play's production exists. Its episodes warn of the abusive powers of yeomen ushers who controlled access to the king and argue for the maintenance of monarchical independence. The play reveals that the court was acting as a forum for debate and that playwrights were able to participate in that process.

According to Walker, a king, like Henry, "who was the plaything of faction," as he has sometimes been described, "would hardly have warranted" attention of this kind from these authors (p. 228). Walker thus reiterates a theme he has touched on elsewhere ("The 'Expulsion of the Minions' of 1519 Reconsidered," *Historical Journal* 32 [1989], 1–16) about Henry's control of his own household. Scholars will find this a fascinating volume even though they may disagree with some of its points, for it suggests intriguing answers to provocative questions about court politics.

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NICHOLAS LOSSKY. *Lancelot Andrewes, the Preacher (1555–1626): The Origins of the Mystical Theology of the Church of England.* Translated by ANDREW LOUTH. New York: Clarendon Press of Oxford University Press. 1991. Pp. 377. $89.00.

The publication of Nicholas Lossky's study of the theology of Lancelot Andrewes's Jacobean court sermons is something of an event. The book carries the blessing of a former archbishop of Canterbury in a foreword because it demonstrates the common catholicity of Anglicanism and Eastern Orthodoxy, and it is further commended in an afterword by a learned canon for illuminating the continuity of Andrewes's theology with both the Continental reformed and humanist traditions. Lossky analyzes Andrewes's court sermons to show how Andrewes, a brilliant classicist and linguist, played a leading role in the recovery of the Greek patristic heritage for the Anglican church.

Andrewes was also reputed to be the most popular court preacher of the reign of James I and enjoyed the unwavering patronage of the king from 1605, when he became bishop of Chichester, until 1624, when his health began to fail. James welcomed the unqualified advocacy of his favorite court preacher for the divine right of kings, and besides having Andrewes preach on the important days of the liturgical calendar, the king also commanded him to preach on the anniversaries of the Gowrie Conspiracy (August 5) and the Gunpowder Plot (November 5). To celebrate his deliverance, James usually got drunk on August 5, but at least he began the day well: it was remarked that the presence of Bishop Andrewes usually had a beneficial effect on the otherwise coarse behavior and speech of the king.

The purpose of court sermons at such political festivals was to demonstrate God's invariable punishment of rebels. Andrewes, like many of his contemporaries, had serious reservations about the veracity of the king's unsupported version of the Gowrie Conspiracy, yet summer after summer he denounced the Gowrie brothers as "treacherous wretches" (Paul A. Welsby, *Lancelot Andrewes, 1555–1626* [1964], 143) and prayed for the "destruction of all enemies of the king" (Lossky, p. 290). Lossky pays scant attention to the historical context of Andrewes's court sermons or their political ideology. The audience at court was more alert to such matters. King Charles I ordered the sermons reprinted after Andrewes's death and, on the eve of his execution, especially commended them to Princess Elizabeth. But Lossky looks beyond such mundane matters to search out evidence of God's mercy and Andrewes's patristic learning.

Archbishop Laud, who was much influenced by Andrewes in both theological and liturgical matters, got into fatal trouble for imposing traditionalist liturgical observances on Puritan parishes. Andrewes also felt very strongly about traditionalist liturgy and decent behavior in church. The difference between the two prelates is that Laud enforced his liturgical practices and Andrewes did not. Andrewes was too much the scholar and court preacher to be an effective diocesan administrator. Consequently, Andrewes died peacefully in his own bed, while his admirer died on the block.

ROGER B. MANNING
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IAN W. ARCHER. *The Pursuit of Stability: Social Relations in Elizabethan London.* (Cambridge Studies in Early Modern British History.) New York: Cambridge University Press. 1991. Pp. xvi, 307. $49.50.

Ian W. Archer has written an important study of how stability was maintained in Elizabethan London. By focusing on social relations, he argues that the capital's stability was at times tenuous and hid serious tensions. The attempt to view London society from the bottom up as well as from the top down provides a counter to the emphasis on harmony and consensus in the work of other recent scholars, notably Valerie Pearl and Steve Rappaport.

Archer begins by looking at the disturbances of the 1590s—a time admitted by all to be difficult. He uses this analysis to set up his model of social relations. He argues that in 1595 the elite felt a real sense of crisis. This perception is critical for Archer, as is the way the riots were used as part of a process of negotiation between rulers and ruled. Although the people of Elizabethan London never rejected the authority of their governors, neither could the elite ever take the support of the people for granted. The dialogue between governors and governed is the centerpiece of Archer's book. The chapters that follow examine the elite, the structures of local government, neighborhood, and community; the livery companies; social policy; and crime and society. Throughout, Archer focuses on the dynamics of the exercise of power, both who had it and their relationship with those who did not. This is a complex and important argument, supported by evidence from a wide range of sources. Archer is a sensitive but critical reader with an eye for the telling detail: the bequest to a parish made on the condition of the creation of a select vestry illuminates the narrowing of the group that made decisions in parishes (p. 71). It is also a courageous argument, given the emphasis on consensus and harmony in so much recent scholarship.

Archer's attempt to bring conflict back into the picture while acknowledging stability, however, also contributes to some of the weaknesses of the book: it is heavy reading, and the argument is sometimes so buried in details that it gets lost. At times, complexity is in danger of becoming simply fence-sitting, and Archer occasionally follows the revisionists in setting anachronistic standards for evaluating conflict (for example, pp. 141, 145). Many of these problems would have been reduced by more energetic cutting

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in the transition from dissertation to book; it might have been easier to read and the argument could have emerged more effectively. Archer also assumes an extensive knowledge of the organization of the City government, its geography, and the overall context of the period: there is no general introduction to the context, and the first—inadequate—map appears on page 151. The one conceptual weakness of the book is that social relations are defined as existing between adult men; relations within the household are scarcely touched on, and Archer fails to look systematically at issues of gender, although the absence of women from most sources itself reflects important aspects of social relations in London society. As a result, there is no context for the limited evidence on apprentices or widows, or the fascinating discussion of prostitution.

Still, one regrets the problems of the book because they tend to obscure—and to render less accessible—an important and significant argument. Archer has developed a sophisticated conceptual model for understanding social relations in the early modern period that offers a compelling alternative to recent interpretations.

Susan Dwyer Amussen
Union Institute Graduate School

Anne Laurence. *Parliamentary Army Chaplains, 1642–1651.* (Royal Historical Society Studies in History, number 59.) Wolfeboro, N.H.: Boydell, for the Royal Historical Society, London. 1990. Pp. 199. $61.00.

More than thirty years ago in *Saints in Arms: Puritanism and Democracy in Cromwell's Army* (1959), I challenged the then-popular conception that radical religious ideas necessarily produced radical political ideas in the 1690s. In *The Rise of the New Model Army* (1981), Mark Kishlansky indicated that I had identified only nine "saints in arms" (p. 1), or New Model Army chaplains, from which number Hugh Peters should have been excluded. He also stated that these chaplains had included Presbyterians as well as Independents and sectarians, that their religious roles had been "overplayed" (p. 91), and that they had represented diverse religious and political beliefs. On the last point Anne Laurence, in her stimulating new book, essentially agrees with Kishlansky that the New Model Army was "pluralist" (p. 85). Her research, however, indicates that there were only nine New Model Army chaplains, including Peters, in 1645 (increasing to eighteen in 1648); that throughout that period they were mostly Independents, with three not too dissimilar Baptists replacing the Presbyterians; and that accounts of the religiosity of the army came mostly from contemporary Presbyterian observers, not Independents or sectarians as might be expected. All six chaplains who served with General Fairfax at the army headquarters were Independents, she indicates, with the possible exception of Edward Bowles.

Over the decade her book covers, Laurence has identified approximately 260 official parliamentary chaplains—exclusive of occasional "mechanik" preachers such as the future Quakers James Naylor and John Bunyan—assigned to the various parliamentary armies or garrisons. The armies included those of Essex, Waller, the Eastern Association, those for the provinces (both northern and western), those for Ireland and Scotland, and, of course, the New Model. In addition to separate narrative chapters, Laurence has included extensive tables of the chaplains' names, denominations, dates of service, and the commanding regimental colonels. She has also supplied mini-biographies of each of the army chaplains, dividing them into those who were paid or served and those who were officially appointed but apparently did not serve. These carefully researched biographical sketches are an inestimable boon to scholars. We find, for example, that among the chaplains were several future Ranters (Abiezer Coppe, Laurence Clarkson, and Joseph Salmon) and future Fifth Monarchists (Vavasor Powell, George Cockayne, and John Pendarves).

In a concluding chapter on religion and radicalism, Laurence touches on the importance of religious toleration, millenarianism, and antinomianism among such New Model Army chaplains as William Dell, John Saltmarsh, William Erbury, and Thomas Collier. I must say, however, that these chaplains actually eschewed denominational or sectarian preferences, including Independency. Although Laurence has not attempted to analyze the available sermons and tracts of these army chaplains, that literature indicates widespread endorsement of antinomian toleration and millenarianism by a substantial number of them. Also, most of the New Model Army chaplains showed some sympathy for their old comrades-in-arms, the soldiers sympathetic to the Leveller movement. Laurence rightly assumes, nevertheless, that there was no necessary ideological connection between antinomian religious radicalism and Leveller political radicalism. She wisely contends that the armies were analogous to the "civilian church in which lay patronage [by the colonels] was the principal influence in the individual appointments of chaplains" (p. 86). I would add that in the selection of chaplains for the New Model Army's regiments, and particularly for its headquarters, the colonels must have felt the strong arm of that tolerationist proto-antinomian Oliver Cromwell, whose influence remained strong in the parliamentary forces.

Leo F. Solt
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David Lemmings. *Gentlemen and Barristers: The Inns of Court and the English Bar, 1680–1730.* (Oxford Histor-

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ical Monographs.) New York: Clarendon Press of Oxford University Press. 1990. Pp. xiv, 323. $64.00.

The history of the English legal profession has blossomed in the last twenty years. The work of Wilfrid Prest, C. W. Brooks, J. H. Baker, and E. W. Ives has given us a full and multifaceted portrait of the common lawyers up until the middle of the seventeenth century, while Daniel Duman has investigated the more modern profession of the eighteenth and nineteenth centuries. David Lemmings's finely crafted monograph on the inns of court and the bar during the period from 1680 to 1730 does more than simply fill a chronological gap in this literature. He demonstrates that this was an important transitional period both for the institutions where the common lawyers received their training and for the profession to which they belonged.

The transition was most evident in the inns of court, which suffered an irreversible decline during this period and for all practical purposes lost control of the profession. Not only did the readings and legal exercises come to a complete halt, leaving young lawyers to acquire their professional training by other means, but the inns also experienced a "crisis of non-residence" that forced them to look outside the practicing bar for lodgers (p. 258). Even the technical call to the bar meant less than it had in the past, since a large percentage of those called never became active professionally. The net effect of all this was the almost complete separation of the eighteenth-century practicing bar from its institutional base.

Lemmings considers these developments beneficial to the profession, which he sees as having undergone an "invigorating purge" (p. 110). The practicing bar became "leaner, fitter, more professional, and more respected," a change that he attributes mainly to the escape of the barristers from the medieval structure and the "enervating atmosphere" of the inns (p. 259). In keeping with this theme, he describes the new breed of lawyers as dynamic and individualistic, indicating that the bar was becoming more open to talent. The change can also be seen in the pattern of preferment within the profession as election to the bench of an inn became less important for promotion to judicial office and as the crown counsel who served in parliament received an increasingly large share of the most important judicial appointments.

Although Lemmings displays admirable caution in describing the changes that the profession underwent during these years, he sometimes appears too eager to identify the successful common lawyers of the Augustan age with the progressive forces within English society. That early eighteenth-century lawyers adapted to the dictates of a commercial society and increasingly served the needs of non-aristocratic groups within English society cannot be denied. They may even have been more responsive to economic change than members of other professions, as Lemmings suggests. But there is very little evidence that the lawyers who took advantage of these changing conditions tended to come from "unconventional backgrounds" (p. 176). Nor can it be demonstrated that the new opportunities to do legal work in connection with commerce and industry encouraged the recruitment of lawyers from the urban bourgeoisie. Indeed, the evidence Lemmings presents for a significant increase in the number of lawyers from socially humble backgrounds is not too compelling. On Lemmings's own showing, moreover, the lawyers of this period often displayed a strong attachment to the mores and economic values of landed society, even when they themselves had not been born into it.

BRIAN P. LEVACK
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NICHOLAS ROGERS. *Whigs and Cities: Popular Politics in the Age of Walpole and Pitt.* New York: Clarendon Press of Oxford University Press. 1989. Pp. xi, 440. $72.00.

Author of the latest in a series of important monographs to challenge the Namierite stereotype of early Hanoverian England as an era of political stability periodically punctuated by squabbling within the political establishment over place and preferment but bereft of a "genuinely popular movement for political reform" (p. 3), Nicholas Rogers also takes issue with revisionist scholars such as Eveline Cruickshanks, J. C. D. Clark, and Linda Colley for their tendencies to deflate the "coherence and political complexity of urban radicalism" (p. 247) and to depict popular politics as a phenomenon controlled by elites. Contending that historians have been "fixated by the inexorable growth of oligarchy" and "inattentive" to "dissident voices" other than the commonwealthmen (p. 246), Rogers persuasively argues that cities "constituted critical sites for a popular challenge to the Whig regime" at "important junctures in Hanoverian politics" (p. 5).

Rogers, demonstrating an impressive command of primary sources, including voting and corporation records, instructions to MPs, pamphlets, newspapers, and an array of manuscript materials, characterizes eighteenth-century towns as "open" and "independent" with "active electorates whose political horizons were both local and national" (p. 5). His data demonstrate that those with populations exceeding one thousand experienced more frequent electoral contests during the early Hanoverian decades than in the Augustan era. Indeed, except for boroughs with declining economies, gentry domination of provincial towns diminished during the century. Although initially predisposed to support the Whigs as bulwarks of the Protestant succession, by the 1730s many large boroughs had been alienated by ministerial efforts to circumscribe popular electoral participation and by Robert Walpole's Eurocentric foreign and fiscal policies, which seemingly favored Hanover and pluto-

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crats. Nationalistic fervor, incipient imperialism, laissez-faire transatlantic commercial interests, and resentment of political oligarchy combined to fuel a Country movement in urban politics that Rogers rightly argues cannot be encapsulated by a crypto-Jacobite label. "National in scope and radical in character" (p. 8), the movement reflected the "groundswell of commercial opinion which saw legislative change as the only security against a ministry too preoccupied with dynastic stability and its own preferment" (p. 249).

Focusing on Bristol, Norwich, and metropolitan London, the "centre of extra-parliamentary opinion" and "springboard for nation-wide campaigns" (p. 5), Rogers deftly navigates the convoluted course of popular politics in these constituencies, concluding that the "boundaries of the informal political nation had broadened dramatically" (p. 385) by the Wilkite era. Although after Walpole's resignation oppositionist politics were undercut by the patriotic response to the "Forty-five" and by Henry Pelham's policies that removed the "stigma of privilege" (p. 255) from fiscal policy and made government appear less monopolized by Whigs, national issues such as the Jewish Naturalization Bill or the loss of Minorca continued to incite passions and provoke popular political initiatives.

One of Rogers's most stimulating chapters analyzes the pre-Wilkite crowd and the place of popular festival in the political spectrum. Although acknowledging that at this juncture crowds were reactive and "traditional, not reformist" (p. 9), Rogers rejects the notion that they were captive to the propaganda and agendas of elites. Noting the "cultural affinities of riot and ceremony," he characterizes street politics as a "terrain" where "ideology, culture, and power" converged (pp. 350–51). "Closely synchronized to a well-defined but flexible calendar of political anniversaries or to the electoral process" (p. 386), crowd actions could periodically disrupt aristocratic government.

Eschewing glib generalizations in his effort to "redraw the psephological and ideological map of Georgian England" (p. 8), Rogers has substantially contributed to our understanding of the complexities and contradictions of the political landscape in the early Hanoverian period. In the process he has demonstrated the "continuities" (p. 406) of the eighteenth-century reform movement. His work will be an important guide for future exploration of the urban political terrain.

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H. M. SCOTT. *British Foreign Policy in the Age of the American Revolution*. New York: Clarendon Press of Oxford University Press. 1990. Pp. xiii, 377. $89.00.

In the general sweep of Britain's European diplomacy in the eighteenth century, the period between the two treaties of Paris (1763 and 1783) marked a nadir in British influence, diplomatic vigor, and prestige. H. M. Scott's meaty volume does much to document the reasons for these characteristics, and more importantly adds texture and subtleties that suggest that there was a great deal more to the story than we have previously understood.

The end of the Seven Years' War and the accession of George III witnessed British influence and power at a height that was not to return fully until the defeat of Napoleon two generations later. The old conflicts that had energized European diplomacy in the first half of the eighteenth century left Britain preeminent at the same time that it lost some of the defining qualities of a European state system. Particularly important, as Scott notes, was the emergence at midcentury of new powers in Eastern and Central Europe. This change transformed the nature of European diplomacy generally and helped to marginalize British influence perhaps as profoundly as they posed a new type of threat to the security of France.

The events of this period are told here in precise and intricate detail. Scott takes exception to the works of Jeremy Black (*Ancient and National Enemies: Anglo-French Relations in the Eighteenth Century* [1986]), among others, which have argued that the domestic dimensions of British foreign policy were central to Britain's relations with the continental powers in the reigns of the first two Georges. Things changed noticeably after 1763. In the place of the old patterns of royal influence, the rage of the press, and the concerns of Parliament, there emerged a new order, one in which the international dimensions of the diplomatic events weighed more heavily than the domestic. Of course both perspectives continued to work on every issue, but purely domestic concerns had less influence than in the past. This change was especially evident after the Earl of Rochford, a career diplomat, became secretary of state in 1768.

The process of squeezing Britain out of a central role in European diplomacy began in the 1760s after the fall of Grenville's ministry. This was largely because his successors possessed limited vision and less competence in foreign policy. Political instability at home, coupled with indifference and inattention to Europe, led to feebleness of policy precisely at the moment when the rise of the Eastern European powers made the old system of security through continental alliances less possible than it had been in the past. Remarkably, British leaders failed to perceive the depth and extent of these changes, although astute continental leaders such as the Austrian Chancellor Prince Kaunitz fully appreciated them.

Perhaps Scott's most interesting and telling chapters deal with British diplomacy during the war with the American colonies. This story is told refreshingly from the European perspective, in the tradition of revisionist accounts, such as R. W. Van Alstyne's *Empire and Independence: The International History of the American Revolution* (1965), which appeared more

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than two decades ago and have not found much favor since. By emphasizing the central concern with the Bourbon powers in the 1770s, Scott makes clear that the Cabinet's preoccupation with France is the key to understanding British policy at this moment of isolation in Europe. The defeat in America, far from destroying British influence in Europe, set Britain free to resume the more important role that would result from the war with France which began a decade after this study concludes.

This volume will long be used as a key reference to the complexities and subtleties of British diplomacy in the critical middle years of George III's reign. It marks these as years in which many of the threads of the triumphant European leadership signaled by the victory of 1815 were first woven, albeit tentatively, into the fabric of British foreign policy.

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KEITH HAMILTON. *Bertie of Thame: Edwardian Ambassador.* (Royal Historical Society Studies in History, number 60.) Rochester, N.Y.: Boydell, for Royal Historical Society, London. 1990. Pp. x, 436. $73.00.

The clerks of the Victorian Foreign Office have come down to us with a reputation for such dilatory behavior that it was said that they played like the fountains in Trafalgar Square, from twelve till four. The subject of Keith Hamilton's biography, Francis Bertie, started as a clerk in 1863 while still a teenager and eventually became Great Britain's preeminent Edwardian ambassador. He served in Paris from 1905 to 1918, thereby completing a career of public service lasting fifty-five years: he was never a time server.

Intelligent, hard working, garrulous, ribald, and increasingly eccentric in old age, Bertie was, with his white hair, rosy complexion, and high hat, unmistakably the epitome of Britishness at the rue Faubourg St-Honoré. If the French have come to expect their English lords to be eccentric, what can the English themselves have made of his funeral cortege, with the coffin hauled off to the cemetery on a hay wagon drawn by Bertie's estate workers dressed in smocks?

Great ambassadors are supposed to have died with the coming of the telegraph and the telephone. The Cabinet and Foreign Office made policy in London and diplomats merely executed what others had decided. As Hamilton's biography makes abundantly clear, the communications revolution actually made it possible for the Paris embassy to participate more fully in the continuous Edwardian debate on the new course in British foreign policy. Bertie, while still in the Foreign Office, had played a significant role in the creation of the Anglo-Japanese alliance; along with Eyre Crowe and Sir Charles Hardinge, he championed the entente with France as a means of combating the diplomatic pressures of Germany's *Weltpolitik*. The greater part of Hamilton's book is concerned with this aspect of Bertie's work; in this sense it is less a biography than a detailed examination of the day-to-day frustrations of keeping France straight in the face of German pressure. What Hamilton makes abundantly clear is that British policy was not, as Keith Wilson has argued (*The Policy of the Entente* [1984]), determined by imperial considerations, but by the European balance of power.

This book originated long ago as a thesis supervised by the late W. N. Medlicott at the London School of Economics. A new collection of Bertie papers gave the opportunity for the revision needed to turn the thesis into the book. The result is an exhaustive, complex, almost forensic analysis of an extraordinary range of primary sources. This is an important book and one that scholars seeking to understand British policy in the era before World War I will find indispensable.

RAYMOND A. JONES
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CAROLYN A. CONLEY. *The Unwritten Law: Criminal Justice in Victorian Kent.* New York: Oxford University Press. 1991. Pp. ix, 244. $29.95.

The title of Carolyn A. Conley's study might suggest that it is primarily a legal and administrative history, but it is in fact of much broader scope. By examining criminal court records and local newspaper accounts of criminal trials, Conley has uncovered material on such diverse aspects of Victorian social history as baby farming, homosexuality, bestiality, prostitution, wife and child abuse, compulsory smallpox vaccination, suicide, xenophobia, Guy Fawkes Day celebrations, election day riots, temperance, and Sabbatarianism. Focusing on the county of Kent between 1859 and 1880, Conley is careful to point out the extent to which one can generalize from such evidence about Britain as a whole, and what was distinctive to Kent.

Conley's main argument is that, despite the efforts of Victorian lawmakers to regularize and equalize the administration of justice, local legal customs based on class and gender inequality persisted. Inequalities were also perpetuated according to the supposed "respectability" of both the accused and the victim. These conclusions are of course not startling; similar "unwritten laws" still shape our judicial systems today. Conley's research reveals, however, how deeply these perhaps ubiquitous inequalities permeated late-Victorian society.

A recurrent theme throughout the book is the extent to which women of all classes were discriminated against in the criminal justice system of Victorian Kent. Women convicted of crimes were usually given much harsher penalties than were men. Crimes against women were considered less serious than crimes against men. The death sentence was much more frequently given when the victim was male

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rather than female. A victimized woman could usually receive redress only if her behavior was submissive and seemingly helpless. Conley points out that an abused wife, for example, was in a particularly difficult legal position: "If she fought back she could be convicted and would likely face a longer sentence than her abuser; if she took the man to court the judge might well decide that she had provoked the violence by not behaving in a properly submissive manner" (p. 72). If a woman was seen as a nag or negligent in household duties, the courts tended to rule that the abuse was warranted. Domestic homicide was rarely treated as murder. These data certainly help counter the arguments of such historians as Edward Shorter, in his *The Making of the Modern Family* (1976), and John Gillis, in his *For Better For Worse: British Marriages, 1600 to the Present* (1985), that wife abuse was no longer socially acceptable in England by the late nineteenth century.

The judicial treatment of women is ironic in light of the Victorian ideology of chivalry. The harsh treatment of children, who were usually so sentimentalized in Victorian culture, is similarly ironic. The courts were reluctant to interfere in the family by prosecuting child abuse, and the murder of infants was seen as much less serious than the murder of adult men. Little girls subject to sexual assault met the same suspicion as did adult women. Despite the Victorian belief in childhood innocence, girls were accused of fabricating the stories or of enticing their attackers.

Conley's careful analysis of such material gleaned from the criminal court records makes this book a valuable contribution to Victorian social history. The weaknesses of the book are minor. Its organization, for example, with a chapter on "violence" separate from the chapter on "women," creates repetition. The narrative is burdened with too many direct quotes from other historians. Finally, so many different topics are covered in this short volume that depth is sacrificed for breadth. Nevertheless, this comprehensive study provides a rich resource for other more specialized historical studies, while offering a fascinating mosaic of late-Victorian society, shaped by the values of hierarchy, patriarchy, and respectability.

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Janet Oppenheim. *"Shattered Nerves": Doctors, Patients, and Depression in Victorian England.* New York: Oxford University Press. 1991. Pp. viii, 388. $27.95.

In this commendable book by Janet Oppenheim, the Victorian sad take their historiographical place with the better-studied mad and bad. For the last fifteen years lunacy, in a scholarly sense, has fascinated social and intellectual historians. We have fine studies of asylums, both public and private, of patient populations ranging from the demented to the criminally insane, and of institutional physicians and the theories by which they attempted to make sense of the disordered mind. Oppenheim, however, breaks new ground by studying what one frustrated Victorian physician, Sir James Crichton-Browne, referred to as the "half-mad": the emotionally troubled beyond the asylum's walls.

To the Victorians nervous breakdown was not a synonym for insanity, conceived of as a mental disease, but rather had an ambiguous but clearly somatic origin in a malfunctioning nervous system. The pivotal feature was an inability to exercise the will, rendering the neurasthenic unable to undertake normal mental or physical tasks. This spiritual lethargy reflected a depletion of nerve force, an amorphous but utilitarian notion of Victorian neurophysiology. Male sufferers, often exhibiting hypochondriasis, induced their own disease by squandering energy in excess work or profligate behavior. In contrast, women, who frequently demonstrated hysterical behavior, bore no responsibility but were prisoners of their fragile reproductive organs.

This model of nervous disorders served a variety of functions. First, it allowed any number of articulate Victorians to make sense of their own dispirited and troubled emotions. Second, for asylum physicians, it provided a window of professional opportunity by which to leave the institution with its poor and usually very disturbed inhabitants in favor of an affluent, ambulatory clientele. Oppenheim's excellent account of the career of Crichton-Browne clearly illustrates the flight from asylum to a lucrative private consultancy. Third, it provided a rationale for the eclectic grab bag of Victorian therapeutics—tonics, cathartics, massage, electrotherapy, exercise, diets, hydrotherapy—all designed to restore depleted nerve force. Finally, when linked to notions of evolution, it created the concept of hereditary neurological degeneration, the prevention of which became a question of national importance on which nerve doctors were thought best qualified to comment.

The nineteenth-century concept of nervous breakdown gradually faltered with the "discovery" of the dynamic unconscious and the consequent relegation of will largely to theology. Fortunately, Oppenheim has salvaged this neuropsychological Victoriana from obscurity in a most readable book. The research on which it is based cuts an enormous swath through English medical and literary primary sources from mid-century to World War I. The secondary literature is surveyed and often, as in the case of gender history or social control theory, subjected to pungent criticism. The result is a volume as sophisticated in its analysis as it is detailed in its contents. It is a work that fills a lacuna in the literature and by virtue of its meticulous scholarship will not soon be supplanted.

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CHRIS WATERS. *British Socialists and the Politics of Popular Culture, 1884–1914*. Stanford: Stanford University Press. 1990. Pp. 252. $35.00.

The pioneering works of R. W. Malcolmson, Hugh Cunningham, and Peter Bailey sufficiently documented the early nineteenth-century battle over popular recreational culture in Britain's new industrial towns and cities. Bourgeois lords of leisure urged "rational recreation" on the populace and provided edifying literature, lectures, music, and seaside outings as counter-attractions to the pub for the improvement of the minds and morals of all. During the final quarter of the nineteenth century, the concern for wholesome leisure activities was voiced with an urgency heightened by the shortening of the work week and the rapid rise of the entertainment industry.

Chris Waters demonstrates in this study that the rhetoric of rational recreation survived right up until World War I, in socialist as well as Tory and liberal circles. The term "elevation" replaced "improvement," but the didactic message remained the same. With dreary consistency, socialists as diverse as William Morris and Keir Hardie warned that commercial entertainment would destroy the character and blunt the political sensibilities of British working people. Habitual attendance at music halls, Association football matches, and seaside arcades were presumed to dupe people into becoming slaves to instant gratification and superficial amusement, filling their minds with "things that don't matter" (p. 35). One would expect (and often got) this sort of elitist drivel from the conservative journalists of the day; in fact, however, this particular judgment came from the pen of an Independent Labour Party spokesman in 1908. "In short," Waters concludes, "socialists never escaped that great Victorian mission to make workers 'moral' beings" (p. 191).

This book charts the reform effort in the realm of popular culture as it was expressed in utopian fiction, arguments with philanthropists, comments about the importance of music for socialist consciousness, and debates over the recreational programs and facilities sponsored by municipal governments. To be sure, this story is not about actual working-class life and leisure, nor is it about the pleasures that arguably led to what Gareth Stedman-Jones called the "remaking" of the English working class ("Working-Class Culture and Working-Class Politics in London, 1870–1900: Notes on the Remaking of a Working Class," *Journal of Social History* 7 [1974]). Instead, it is about the ideologues and their schemes, the would-be managers and their intended work: socialist intellectuals who talked, on the one hand, about the inalienable right of the working class to define their own values and to choose their own destinies, but who sought on the other hand to shape working-class behavior into forms acceptable to middle-class—albeit socialist middle-class—opinion.

Ultimately the effort failed simply because workers wanted something different; they refused to repress their enthusiasm for football matches and music halls in favor of more "elevating" pursuits. Even the best-intentioned schemes of leisure reform had "an air of charity and goody" about them, "which the poor always regard with suspicion," noted George Sims, a refreshingly candid late-Victorian. "They want their amusement as a right, not as a favour, and they decline to be patronised" (p. 69). Little wonder that workers decisively turned away from socialism to the leisure industry, from prescribed culture to popular culture, and that socialists turned away from the working class to a Labour Party that would furnish, through the state, an elevated and elevating national culture.

This book is admirably researched, drawing on unpublished documents, periodicals and newspaper accounts, and published pamphlets and books. The prose is competent, the judgments fair. Unfortunately, there is a certain dreariness that hangs over the narrative, largely the result of the ponderous arguments and pontifications of the principal characters. Yet the author must take some of the responsibility. Surely some liveliness intruded in the form of working-class resistance to socialist prescriptions. Huge crowds at football matches, for example, protested against elitist (socialist and otherwise) concerns with their feet, their dress, and their voices, not to mention their drink. That lively aspect of popular culture is nicely documented in Tony Mason's *Association Football and English Society 1863–1915* (1980), a book that Waters barely mentions in a footnote.

WILLIAM J. BAKER
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BARRY WEBB. *Edmund Blunden: A Biography*. New Haven: Yale University Press. 1990. Pp. xiii, 360. $35.00.

Edmund Blunden has a permanent place in the front ranks of English soldier-poets of World War I, alongside Siegfried Sassoon, Robert Graves, Wilfred Owen, and Isaac Rosenberg. It is generally agreed that Rosenberg was one of the two great poets killed in the war; the other was Owen. Blunden, Graves, and Sassoon were survivors of that carnage, at a considerable psychological cost. As a writer, Graves grew in stature in the postwar years; Blunden and Sassoon less vividly so. One has the sense with them of promises not quite fulfilled, of achievements falling short of what might have been. It is this undercurrent, perhaps not consciously intended, that lends a special interest to Barry Webb's biography of Blunden. His life, as we see it here, suggests both the continuity of English letters across the centuries and its special twentieth-century characteristics, shaped to a considerable degree by World War I.

Although he was born in London, his parents, who

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were schoolteachers, moved to the countryside in Kent when Blunden was four, and most of his poetry would be intensely rural. He was a precocious poet who did very well at his school, Christ's Hospital, where he acquired a permanent interest in the best-known writers who had gone to the school before him: Charles Lamb, Leigh Hunt, and Samuel Taylor Coleridge. He was destined for the career that in fact he had: as a literary figure who would write an extraordinary number of poems, reviews, literary studies, not to mention thousands of letters. He would also be deeply involved in the business of literature, most notably serving on the editorial staff of the *Times Literary Supplement*. He had just a year as an Oxford undergraduate after the war, yet from 1931 to 1945 he was a successful English don at Merton, and toward the end of his life he was elected professor of Poetry there, but he had to resign because of ill-health. He also played a role in bringing British culture to the Far East, as a professor of literature in Japan in the 1920s, then again shortly after the end of World War II, and finally from 1953 to 1964 at the University of Hong Kong. Despite the extraordinary range of his poetic activities and his critical and biographical writings, he is best known—deservedly so—for *Undertones of War* (1928), a classic account of his life at the front during World War I. Although he was a friend of Sassoon and Graves and the editor of the first edition of the poetry of Wilfred Owen, he had a long estrangement from Graves after the publication of *Goodbye to All That* (1929), Graves's war account, for he felt it was untruthful. His own less caustic, beautifully written war book was the story of a pastoralist in the trenches: "simple joys with the ironic cruelty" (p. 97). Blunden characterized himself, in the last line of the book, as "a harmless young shepherd in a soldier's coat."

Webb has provided a well-written, intelligent account. There is, however, virtually no criticism of Blunden's writings nor much analysis of his life. Other than his restlessness and his inability to say no, there is no particular explanation of his quite striking personal life. He married his first wife, Mary, who was from a rural laborer's family, when he was 21. He had an intense relationship with Aki Hayashi when he was in Japan in the 1920s, and although his passion for her had cooled, he brought her back to England in 1927. She remained in London until her death in 1962, apparently friendless, supported by Blunden, who was never well off, serving as his research assistant. He was also deeply involved with his sister-in-law, Annie. His second wife was a writer of Armenian extraction, Sylva Norman. Although he did not divorce her until 1945, from 1939 on his deepest involvement was with Claire Poynting, twenty-two years his junior, whom he married after his divorce. He maintained close relationships of varying intensity with all these women, writing them innumerable letters, as well as conducting a vast literary correspondence and following a punishing schedule that included 600 lectures during the three years he was in Japan in the late 1940s. He finally collapsed and during his last five years barely wrote or spoke.

Webb has written a fascinating account about a significant English figure. He leaves it to the reader to decide what Blunden's life meant and what were its driving forces. Blunden appears to have been intensely loyal to almost all institutions, places, and individuals that he knew. One would have welcomed more discussion of the implicit collision in his life: the traditional English man of letters, stylishly turning out prose and poetry, hunting for used books, playing and loving cricket, and teaching the young, over against the traumatic experience of surviving the trenches of France. It was that experience, after all, that inspired the work for which he is justly celebrated.

PETER STANSKY
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ERIC HOPKINS. *The Rise and Decline of the English Working Classes 1918–1990: A Social History*. New York: St. Martin's. 1991. Pp. viii, 295. $39.95.

"In fact the eighties constitute . . . a longer period of mass unemployment than even the 1930s" (p. 221). Admittedly, this grim feature of the British economy was unevenly distributed, both in the country and within the English working classes (a pluralistic construct argued in this book by Eric Hopkins). For those among the working classes who had steady employment, however, there was no fall in the standard of living in that decade; indeed, the better paid among them enjoyed "a lifestyle very little different from the lower middle classes" (p. 195). Thus, the latter-day "decline" of the book's title is a less universal concept than the "rise" of the working classes, a phenomenon described by historians in such particulars as the political emergence of the Labour Party, the advent of the welfare state, and a doubling of the working classes' standard of living in the 1950s and 1960s. For many among the working classes directly affected by adverse developments in the economy, the "decline" of recent years has translated into poverty, not only among the unemployed but also for the sick and disabled, pensioners, single parents—a facet of change in society's mores—and the low paid. Although the numbers in poverty remain in dispute, sufficient documented human misery exists to justify Hopkins's conclusion that the contemporary welfare state was "failing to provide satisfactorily that lifelong security it was set up to achieve" (p. 236).

Such a failure, rooted as much in politics as in economics, has almost surely produced a new underclass among the English working classes, a division further enhanced by the increasing numbers of married women in employment: since married working women were more likely to come from families with working husbands (many of whom drew low wages, as

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did their wives) than from families with unemployed spouses, the income gap between employed working-class families and those drawing benefits grew even wider. Whether or not such a discernible gap creates an updated Disraelian "two nations," the implications of such an underclass are daunting. Yet its existence has been politically exploited by the Thatcherite attack on a "dependency culture," a negative message of no less import for the underclass than the positive message of "privatisation" given to employed workers. Assessing this divergence, Hopkins contends that the years since 1974 have witnessed "a decline in the corporate strength and identity of the working classes as a whole" (p. 278). Such a common "identity," presumably in existence well into the era of Harold Wilson, is difficult to reconcile with the pluralistic structure of the working classes; Hopkins should content himself with arguing that the working classes have grown even less homogenous in recent decades. Hopkins is in any case justified in entitling the two chapters dealing with the post–1974 years "Darkening Horizons." And he has made his most valuable contribution in his description of these troubled decades, even granting his cautionary note that conclusions about the recent past are necessarily more tentative than those at a greater remove.

Ironically, Hopkins's lasting historical contribution may be found precisely where he feels least secure in his argument, for he affords initial interpretation and opens up historical dialogue on these recent decades. In contrast, his earlier chapters (to 1974) cover ground well worked by other historians, often in greater detail. Hopkins's approach differs in his design to write a social history of the working classes within a political framework, mandated, he argues, by the close ties between workers and the Labour movement, in its political and industrial wings. That attempt is not everywhere successful, however, as the political framework is on occasion cluttered, even distracting. Possibly he attempts too much, but his theme is one well worth following given the present-day travail of many in the English working classes.

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ARTHUR J. MARDER *et al. Old Friends, New Enemies: The Royal Navy and the Imperial Japanese Navy*. Volume 2, *The Pacific War, 1942–1945*. New York: Clarendon Press of Oxford University Press. 1990. Pp. xxx, 621. $69.00.

When Arthur J. Marder died at Christmas, 1980, he left this book incomplete. The first volume, published in 1981, ended with the Japanese attack in December 1941 and the loss of the ships *Prince of Wales* and *Repulse*, the symbolic end of the era of British dominance in Southeast Asia. The first six chapters of the book under review were drafted by Marder; the remaining twelve are the work of two of his former graduate students. They have done a job of which their mentor would have approved. The book is a smoothly written and meticulously researched account of the Royal Navy's war in the Indian Ocean and the Pacific. One of the strengths of Marder's previous work was his skill in winkling out the details of those relations between admirals that constitute the inner political history of naval high command in war as in peace. This dimension of the British war effort against Japan is fully explored—necessarily so, since the ambiguities and frustrations of that war bred bitter personality clashes within and between services. The authors describe as "The War Between the Admirals" the quarrel in 1944 between Lord Louis Mountbatten, the supreme allied commander in Southeast Asia, and Sir James Somerville, the commander in chief of the Eastern Fleet, over the exact boundaries of their respective spheres. But this, as the authors correctly point out, was merely the local reflection of a larger, even nastier, tussle between Winston Churchill and the British chiefs of staff over the shape of British strategy in the war against Japan. Churchill wanted an "imperial strategy" based on India and aimed at the restoration of imperial prestige by the reconquest of Singapore. The chiefs of staff wanted a major British contribution to the American drive across the Pacific. It was London's longest, most bitter fight of the war over a strategic issue.

Both sides were, of course, being unrealistic. An independent imperial strategy was not feasible for an increasingly resource-poor Britain and, in any case, the prestige lost between December 1941 and February 1942 was not recoverable. Yet the Americans, particularly the U.S. Navy, neither needed nor wanted British participation in their war against Japan. Churchill managed, at the "Octagon" conference at Quebec in September 1944, to force acceptance of a British Pacific Fleet on the Americans. Handicapped by a lack of resources, especially in the vital category of auxiliaries for a fleet train, and ill-prepared by not having grappled with the problems of trans-Pacific campaigning until 1943, the British Pacific Fleet played an honorable and courageous, but inevitably minor, role in the closing months of the Pacific war. Correlli Barnett has written of the British becoming a "warrior satellite" of the United States by 1945 (*The Collapse of British Power* [1972]). This thorough study of the Royal Navy's role in the war against Japan amply demonstrates the accuracy of that observation.

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KENNETH O. MORGAN. *The People's Peace: British History, 1945–1989*. New York: Oxford University Press. 1990. Pp. xiii, 558. $29.95.

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Although Kenneth O. Morgan's book makes no claim to being the latest volume in the Oxford History of England, it looks very much like an addition to that series. Certainly, as a political narrative of the period, it is a worthy sequel to A. J. P. Taylor's *English History, 1914–1945* (1965). Gracefully written, with sharply etched portraits of both Tory and Labour leaders, it skillfully traces the rise and fall of the different governments since 1945. Although written from the standpoint of a moderate Labour supporter, and filled with often caustic judgments about the weaknesses and failures of governments and their leaders, Morgan strives throughout to give the major political actors their due. His book has two other notable strengths: it pays careful attention to political developments in Wales, Northern Ireland, and Scotland; and it smoothly integrates the cultural history of the period into the narrative by discussing dozens of different novels and plays as well as popular political and sociological analyses of Britain. It is surely the best and most readable narrative political history now available of Britain since World War II.

Nevertheless, although anyone interested in the history of contemporary Britain will want to read this book, it is less than convincing as an analysis of British decline, the main theme of the volume. In Morgan's view, a postwar "era of advance"—when the Keynesian welfare state was put in place, British leaders still had confidence in their ability to govern, and their followers still had faith in their leadership—gave way after 1961 to a "retreat" that culminated in the Thatcher years of "storm and stress." Morgan particularly bemoans the "dissolution" of the "civic culture of Britain" that "had rested . . . on the strength of certain bases, the nuclear family, the coherent neighbourhood, the benevolent virtues of professional advancement, respect for the rule of law" (p. 297).

Aside from the incompetence of various political leaders, Morgan seems to lay much of the blame for this development at the feet of those groups who protested that this civic culture actually rested on rigid class distinctions, inadequate social opportunity, failed political leadership, authoritarian institutional structures, and male dominance. The "overweening power of the TUC" contributed to the "sectionalism" that undermined "Britain's public culture and declining economy" (pp. 304, 255). Student movements "helped on a process of deracination and disenchantment" (p. 296). Women achieved needed reform but helped to "generate social and personal tensions" (p. 356). Not only are these examples of blaming the victim, they fail to locate the real causes of economic and moral decline, which are to be found in larger processes of social and economic change. But changes in the British economy, the significance of Britain's peculiar economic institutions (such as the well-known division between the City and industry), the inadequacies of British industrial leadership, the decline of traditional manufacturing industry, and the consequent remaking of the working class and the stable neighborhoods it once inhabited remain unexamined. Similarly, Morgan underestimates the degree to which Britain's misguided commitment to a world role and to a largely imaginary "special relationship" with the United States contributed to its economic decline. In short, this book is a superb political narrative, but limited as an explanation of what has gone wrong in the past fifty years.

PETER WEILER
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JOHN KENDLE. *Ireland and the Federal Solution: The Debate over the United Kingdom Constitution, 1870–1921*. Buffalo, N.Y.: McGill-Queen's University Press. 1989. $34.95.

John Kendle's book is a meticulously researched contribution to Anglo-Irish and imperial historiography. The author explains those previously recognized factors that stimulated interest in federalism, that is, a broad concern to relieve the parliamentary congestion that accompanied the expansion of central government and a desire to strengthen the links of empire against increasing military and economic competition from Germany, France, and Japan. This study demonstrates the extent to which Irish nationalist demands for self-government, first in the form of Home Rule and later in the form of an independent republic, also stimulated and shaped the federal debate, most specifically during the Home Rule crisis of 1886, the tripartite budget, constitutional and Ulster crisis of 1910–14, and finally the Anglo-Irish War of 1919–21.

Like other political thinkers, William Gladstone was intrigued by the federal systems recently established in Canada and Germany. He believed the Canadian model allowed for an effective balance of national unity with diversity that could be applied to Ireland to satisfy Irish demands for self-government while simultaneously consolidating the empire. When it came to drafting his bill, however, Gladstone was never able to reconcile his federalist leanings with his determination to retain the sovereignty of Westminster. Likewise, the Home Rule all-around schemes suggested for Scotland and Wales in the wake of the Irish debates of 1886 did not encroach on Westminster's legislative supremacy. Kendle shows that there was a widespread tendency for devolutionary schemes such as these to be inaccurately described as federal. Indeed, throughout this era the advocates of federal schemes never adequately dealt with the constitutional and fiscal readjustments that a truly federal system would entail.

Kendle demonstrates that prior to 1910 there was little interest in federal schemes among the predominantly English MPs, nor among the Scottish or Welsh electors. Irish Home Rulers opposed "federalist" Home Rule all-around proposals as inevitably delay-

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ing rather than facilitating the restoration of their national government.

It was not until 1910 that a serious interest in federal proposals for the United Kingdom emerged among the politically influential. Enthusiasm for a federation as a prerequisite to imperial federation was found among Unionists associated with the Round Table movement. Added to the customary motives of relieving parliamentary congestion and strengthening imperial defense was the hope that a U.K. devolved structure would dilute the socialist threat that Tories perceived in the Liberals' cooperation with Labour and the Irish. In autumn 1910, J. L. Garvin of the *Observer* and F. S. Oliver of *The Times* waged both private and press campaigns reviving the Home Rule all-around idea. Such a policy, they believed, would save the House of Lords, satisfy Ireland's claims, and effect the start of imperial reconstruction.

These plans came to nothing because the Tory chief, Arthur Balfour, remained unconvinced that the federalist approach would provide a way out of the Irish difficulty or indeed provide for imperial consolidation. Kendle provides a full description of Balfour's criticism of the vague and ill-defined proposals advanced by Garvin and Oliver. Interest in a U.K. federal devolution resurfaced briefly in 1913 in hopes of averting civil war in Ulster, but this got nowhere given the Ulster Unionists' determination to resist any measures that would alter the Union.

The federal debate died with the outbreak of World War I and was not resurrected until Britain's embarrassment over the Anglo-Irish War in 1919 provided an opportunity for federalists like T. A. Brassey and Murray MacDonald to raise the issue again. Lloyd George appointed a thirty-two member conference to consider federal devolution in the fall of 1919. The members eventually agreed on a distribution of powers between Westminster and subordinate assemblies, but no consensus emerged on whether the assemblies should represent regions or the respective Irish, Scottish, Welsh, and English national groups. Thus, the issue was dropped.

Meanwhile, a separate Cabinet committee, headed by Walter Long and charged with finding an alternative to Home Rule, continued to espouse a federal approach in hopes of maintaining both Irish unity and the Union with Britain. The Government of Ireland Act (1920), with its provision of a Council of Ireland to eventually exercise some authority over the two states created by this legislation, bore the hallmarks of Long's misplaced hopes in federalism. Neither the Irish nationalists nor the Ulster Unionists were interested in developing the federalist implications of this legislation.

Any British interest in federalism ended with the apparent solution of the Irish problem that the legislation of 1920 achieved. Sadly, the last twenty years in Northern Ireland have shown just how inadequate and temporary that solution was. In illustrating the political pitfalls and complications of actually devising a federal constitution that provides for diversity within a larger political structure, Kendle's study will be a useful source for those politicians and civil servants who may look to federal schemes to resolve the Northern Irish problem.

CATHERINE B. SHANNON
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RIDGWAY F. SHINN, JR. *Arthur Berriedale Keith, 1879–1944: The Chief Ornament of Scottish Learning*. Aberdeen: Aberdeen University Press. 1990. Pp. xv, 386. £25.00.

Historians have long known of A. B. Keith's works on British colonial development, management of the empire, and evolution of dominion sovereignty, but almost nothing of the man and his other works. Now Ridgway F. Shinn, Jr., has filled this gap by writing the first scholarly biography of the former colonial administrator, constitutional lawyer, and university professor. The portrait that emerges, drawing heavily on official sources, private correspondence, and personal memorabilia, is that of a man whose actions and thoughts had more influence and impact than one had imagined.

The strength of the biography is that Shinn makes clear the result of his extended research. Assuming almost complete ignorance on the reader's part, Shinn goes to great lengths to put each institutional dispute in the proper context, whether it be yet another difficulty at the Colonial Office or a controversy at the University of Edinburgh. After a brief discussion of Keith's early life at Edinburgh and Oxford, something one would expect of a budding mandarin of Britain's burgeoning empire, we are taken through Keith's career as second secretary of various Colonial Office departments, secretary to the crown agents for the colonies, and junior assistant secretary to various imperial conferences. As an official, Keith took his role and status more seriously than one would expect given his rank and experience, behavior that brought him into conflict with superiors and colonial politicians. It seems that Keith, in theory and practice, was a dutiful student of Albert Dicey's constitutional views, which Keith got to know quite well while taking law degrees at Oxford. Consequently, his career was fatally compromised, especially due to his attempt to dictate the result of a Natal court martial after the restoration of South African self-government.

Keith consoled himself by taking up a scholarly career, becoming a Regius professor of language at Edinburgh in 1914. His apprenticeship for this position, particularly the thesis he wrote on the theory of state succession for his Oxford DCL, gave him a more balanced view of British colonial relations, culminating in the appearance of his classic study, *Responsible Government in the Dominions* (1909). At Edinburgh, the

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still prickly Keith churned out books on British constitutional development and Indian culture, and articles for the *Journal of Comparative Legislation* and *Journal of the Royal Asiatic Society*. The scope and quality of this work are truly staggering and defy any serious analysis in a review of this length. Through his continued dealings with the affairs of India, Canada, Ireland, and South Africa, he was able to describe the full impact, implications, and paradoxes of the coming of the Statute of Westminster and World War II. In retrospect, Keith certainly seems to have gotten the better of the arguments he had with critics and British politicians.

The book suffers from all the standard weaknesses of scholarly biographies. Eager to exploit every bit of new information, except when it occasionally does not reflect well on the man, Shinn does so with too little critical awareness, and often at the expense of what we should really know about the subject. Shinn is particularly desirous of building up Keith's largely secret relation with Dicey, whom he got to know quite well through private correspondence. We are told of their letters about the nature of various British possessions (pp. 137–38) and the nature of dominion rule (pp. 157–59) in a most uncritical fashion. Dicey was not involved here in any scholarly pursuit, but seeking to befriend the chief Liberal constitutional lawyer in the hope of securing his silence about Dicey's claims about the need of the referendum to stop radical policies by the government, a campaign Dicey kicked off with a provocative speech before the British Constitutional Association on March 12, 1909. By the time the Parliament Bill was enacted, Dicey was involved with *The Times* in stopping passage of the third Home Rule Bill no matter what the cost. I would have also preferred to have learned much more about the actual contents of Keith's important works rather than just have had a table-of-contents survey of them, and what any critics thought of them, especially since there is a most extensive bibliography.

In going wrong on Dicey, Shinn is badly served by Richard Cosgrove's biography of him. Acting as if Cosgrove was the last word on Dicey (p. 157), Shinn gives a Diceyan view of the British Constitution in presenting Keith's ideas on the growth of responsible government (pp. 123–26), and a sanitized view of Dicey's crusade against Asquith's ministry. This is amazing, since my biography of Dicey was published in 1985; yet, five years later, Shinn chooses to ignore its existence and most of the articles on which it was based, assuming apparently that reviews dismissing it, like that of Reba Soffer in the *AHR* (91 [1986], 922), were sufficient. Shinn's biography is worse because of it.

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EUGEN WEBER. *My France: Politics, Culture, Myth*. Cambridge: Belknap Press of Harvard University Press. 1991. Pp. 412. $24.95.

Eugen Weber is a national treasure. The only question is which nation. A native of Romania who is almost equally at home in England, France, and the United States, Weber has focused on French history, but he has always examined it with cosmopolitan wit and wisdom. In this volume he has appended a graceful autobiographical note to fourteen of his finest articles. From "New Wine in Old Bottles" to "Revolution? Counterrevolution? What Revolution?" we review his work on French character, popular beliefs, peasant politics, right-wing movements, major intellectual figures, anti-Semitism, and revolution. This polished stylist tells us he took up history because he lacked the competence to pursue science and the imagination to write fiction; if so, history gained from his deficits.

Weber has not adopted big ideas and pursued them relentlessly. Instead he has immersed himself in the materials of the time under investigation and commented wryly on them; he is a fox, not a hedgehog. Accordingly, his larger generalizations are often his most suspect. A case in point is Weber's analysis of peasant politics, first presented in his beautifully documented but oddly argued *Peasants into Frenchmen* (1976). There, and in the two articles on the subject reprinted in this new collection, Weber uses the rich observations of nineteenth-century folklorists, memorialists, and officials first to portray the particularism of French rural life, then to claim (as he now puts it) "that residents of the country, France, lived in different historical time-zones until the late nineteenth century, and that France was only truly unified by the deliberate enterprise of the Third Republic" (p. 10). Weber counters the evidence of deep rural mobilization in 1848–51, arrayed by such historians as Maurice Agulhon, Eugène Ténot, Philippe Vigier, John Merriman, and Ted Margadant, with piecemeal documentation suggesting that nineteenth-century peasants sometimes clothed self-interest and local rivalries in the language of national politics, that elite observers described peasants as ignorant of national politics, that the real leaders of rural action were not usually peasants, and that trivial incidents "could easily acquire a political coloring" (p. 147).

Life is, of course, a web of persons and particularities; Weber recalls that truth vigorously. But at issue is whether and when a set of changes in local-national political connections occurred. The sort of argumentation Weber uses in his two post-*Peasants* articles would equally undermine his own identification of the later Third Republic as the turning point. Such ethnographers as Laurence Wylie, after all, emphasized the particularism of French rural politics as recently as the 1950s (*Village in the Vanchise* [1957]).

Fortunately, Weber's incomparable eye for detail serves him well when he illustrates his characterization of the so-called nationalist revival before 1914: "vague, incoherent, tangible but indefinable—and almost impossible to explain outside the detailed story of events" (p. 204). His luminous, unfootnoted sketch

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of Marc Bloch, first published in the *American Scholar*, demonstrates his sensitivity to nuances of individual experience. Weber's sampler of his past work will make his delighted readers long for at least two more books: a collection of his reflective book reviews and a full-length autobiography of this "incurably curious" chronicler of complexity.

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JOSEPH BERGIN. *The Rise of Richelieu*. New Haven: Yale University Press. 1991. Pp. xiii, 282. $30.00.

Joseph Bergin's books, including an earlier one on the Richelieu fortune, *Cardinal Richelieu: Power and the Pursuit of Wealth* (1985), reflect hard, scrupulous research in such archives as the Paris *minutier central*. In this new work, Cardinal Richelieu's career is traced from youth to ecclesiastical office to minister of state in 1616; then through his fall from power, exile, and reinstatement as Louis XIII's chief minister in 1624. Bergin reaches beyond Parisian sources to British archdiocesan and papal archives, and his discussion is informed by a wide sampling of recent scholarly work.

A thesis underlies this book. The cardinal's skills, Bergin writes, "were not uniquely innate or God-given" (p. 9). This hagiographical view would seem obsolete, yet it is a target of Bergin's arguments. He considers most biographical studies of the cardinal, including psychological studies "nearer to our own time," have mistakenly sought to uncover "factors which, in effect 'programmed' Richelieu for a leadership role in French politics" (p. 2). To support his view, Bergin presents evidence that young Richelieu was typical of his cultural and social milieu at every stage of his unfolding career. Yet this evidence is sometimes doubtful, both in what it includes and in what it omits.

Among the doubtful inclusions is Bergin's use of the so-called *Mémoires* of Richelieu. Despite awareness that this work is a largely posthumously edited compendium of assorted documents, few by the cardinal (pp. 7, 69), Bergin sometimes cites it as "his" account, presumably for lack of a better source (pp. 180, 227, 237). Again, Bergin uses two medical manuscripts to maintain that, as a consequence of a "tempestuous" youth like that of many noble Latin Quarter students of the period (around 1600, when Richelieu was fifteen), by 1605 Richelieu was carrying a sexual disease that "was neither recent nor the result of a chance, unfortunate encounter" (p. 58). Richelieu's illnesses of middle age may indeed represent the progression of an infection acquired in youth, a point not made by Bergin. My reading of the manuscripts Bergin cites, however, leaves me doubtful that the "M. le Cardinal" treated for an uncertain ailment in 1602 was the Richelieu who became a cardinal twenty years later, nor did I find in the margin the note Bergin sees there identifying this person as Richelieu. Neither does it seem likely that the nineteen-year-old theology student, two years short of being confirmed as bishop of Luçon, was the "M. de Lusson" reported to suffer from a long-standing gonorrheal infection in 1605, rather than one of at least three actual Mr. de Lussons mentioned in contemporary writings. Unlike Bergin, I judge that the note identifying Lusson as Richelieu in this manuscript is neither in the same hand as that of the text nor necessarily contemporary.

On the count of omissions, Bergin's view of the Richelieu family history as a virtually unexceptional pattern for second-level nobles with high appointments at court obliges him to minimize or omit contemporary accounts that give an unsavory picture of Richelieu's father. Unmentioned are letters left by the cardinal's widowed mother that show her bitter and knowledgeable struggle to rescue her sons' patrimony. Bergin's picture of an unambiguously positive paternal model leaves no room for conflicts that might help to explain Richelieu's development.

Again, in Bergin's view, Richelieu's doctrine and practice as a new reform bishop were sufficiently indistinguishable in zeal, rigor, orthodoxy, and spirituality from those of his peers to class him among the *dévôts* of his time (p. 114). Bergin does not quote from the many passages in Richelieu's early writing that have struck theologians with their secular, political tone.

Bergin's claim that there is little evidence of distinctive political ambition in the young prelate minimizes Richelieu's deeds, such as his trip to Rome to promote his own candidacy as bishop, his delays in leaving the court to take up duties in a remote diocese, and his ardent efforts to return to the capital. Richelieu's words, however, bespeaking high-flown conceptions of himself in the service of an idealized monarchy and a glorified France, leave even less ambiguity on this point. Had Bergin quoted these, it would have been difficult indeed to sustain an image of his protagonist as another face in the crowd of diligent bishops.

Once Richelieu regained power, Bergin persists in this image: "Neither a savior-figure and statesman who was the exception to every rule, nor a genius who could blithely defy the ordinary laws of political gravity" (p. 262). Straw targets aside, to what should we attribute the powerful effect Richelieu had on contemporaries and his later spectacular performance? Not, according to Bergin, to any special mix between personality, milieu, and exceptional family experience, but chiefly to the political experience he gained in the difficult role of courtier to Marie de Medici during part of her exile. Yet none of the other clerical and the real *dévôts* who shared Richelieu's experience in the queen mother's circle acquired the same skills or perspectives that Richelieu did.

Bergin does add facts to what is known about Richelieu's studies and his appointment as bishop of Luçon in 1607. Familiarity with church administration and history, already disclosed in his work, *Cardi-*

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nal de la Rochefoucauld: Leadership and Reform in the French Church (1987), also enlightens Bergin's discussion of Richelieu's progress as a cleric. His account of Richelieu's early political career is admirably detailed and without equivalent in English for the period from Richelieu's first venture onto the national political scene to his restoration to the high post he held until his death. Interpretations aside, these contributions are more than enough to make this a book from which all historians of early seventeenth-century France will profit.

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Evelyne Berriot-Salvadore. *Les Femmes dans la société française de la Renaissance.* (Histoire des idées et critique littéraire, number 285.) Geneva: Droz, with the cooperation of the Centre des Langues et de la Communication de l'Université de Corse. 1990. Pp. 592.

Did the status of women improve or decline during the Renaissance? Did French women have, to borrow from a well-known essay by Joan Kelly, "a Renaissance"? This is the question that Evelyne Berriot-Salvadore poses at the beginning of her study. As the book's title indicates, its subject is French women and the place accorded them in the years between 1500 and approximately 1614. This is not untrodden territory. Literary scholars and a number of historians, including Natalie Zemon Davis, Joan Kelly, Sara Hanley, Nancy Roelker, and Merry Weisner have already tilled these fields. Still, Berriot-Salvadore's thorough study constitutes a useful supplement to our knowledge.

Berriot-Salvadore does not rely on archival research but rather on an exhaustive reading of literary sources. She uses conduct books, family histories (*livres de raison*), legal treatises, pamphlets, religious writings, and scientific texts to reconstruct cultural attitudes toward women of all social groups. Scholars will find new material in her detailed discussion of women's legal position in both northern and southern France. They will also find her research on women's patronage of literature interesting. One does wish that greater account had been taken of the state in the legal fate of women and that some attention had been paid to the content of women's writings.

Berriot-Salvadore concludes that women saw little improvement in their status during the sixteenth century. A distinctly feminine form of writing, she contends, did appear during this period, but the number of women writers remained small and limited to the elite. In other domains, progress was even less clear. Legally, culturally, and materially women saw their fortunes deteriorate. Women's historians will not be surprised by Berriot-Salvadore's conclusions. This study confirms the suspicion voiced by Kelly almost twenty years ago that European women did not have a Renaissance.

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Leslie Derfler. *Paul Lafargue and the Founding of French Marxism, 1842–1882.* Cambridge: Harvard University Press. 1991. Pp. x, 285. $39.95.

Historians of the French Left used to be much exercised over the question of whether Marxism developed within French radical traditions or was a German import badly grafted onto the much more utopian French radical tree. Leslie Derfler's biography of Paul Lafargue is a contribution to this debate. Derfler uses the story of the early life of Karl Marx's grandson to argue that it was Lafargue who brought the Marxist message to France in the years of the First International.

Derfler's biography narrates Lafargue's early history, from his birth in Cuba to the point at which the Marxist wing of the French Left founded the *Parti Ouvrier.* It must be said that Derfler has written a highly readable account of these years, an account replete with personal details about the young man's encounter with the Marx family in London, his courtship of Laura Marx, and his various political positions (first as a Proudhonist and subsequently as a convinced adherent of Marx's ideas). At the same time, however, historians of the French Left will find much to quarrel with in Derfler's attempt to place Lafargue at the center of French radical politics in these years. First among the author's misinterpretations is his belief that Lafargue was a proponent of women's emancipation. He rests that case on the unexamined assumption that because the Guesdists were early adherents of women's equality, Lafargue must have been as well. But even a cursory reading of Lafargue's speeches to groups of working women, for example, or a quick look at the several books and articles that have treated the relationship between the French Left and working women in this period, would have shown an altogether different Paul Lafargue: a man committed to extremely reactionary ideas about women's "proper" destiny as figures marginal to any kind of public life.

Other historians will find Derfler's attempts to establish Lafargue as a competent theorist of Marxism problematic. Although one might sympathize with Derfler's wish to redeem Lafargue from a widespread historical neglect by arguing that his writings both shaped Marxism in France and prefigured later Marxist ideas (especially those of Gramsci and Lukács), his efforts are unconvincing. Indeed, the work discussed here seems, if anything, to justify Marx's judgment that his son-in-law was not particularly intelligent.

Third, some will find Derfler's account of Lafar-

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gue's "central" place in European radical movements—including the First International—to lack credibility, not least because Derfler himself does not demonstrate a thoroughgoing knowledge of European left-wing politics in the middle decades of the nineteenth century. He has not read, for example, the key works on the First International. Nor has he done very much primary research into the history of First Internationalist groups all over Europe. Had he done so, he would not so readily have concluded that Proudhonism dominated all French First International groups. He might also have come across articulate socialist women leaders in Switzerland, in parts of France, and in Belgium. Proudhon had his challengers—among the French as well as among leaders of the Left from other countries.

And again, Derfler's neglect of research on the constituency claimed by the Guesdists—the workers of the industrial north of France, for example—has led him to blame Lafargue's unpopularity with other French left-wing intellectuals on his subject's putative preference for workers in industrial regions (according to Derfler, Marxism's "natural" adherents). Reading between the lines, one can conclude that Lafargue's contemporaries may have found dozens of more immediate reasons to dislike him. He was arrogant, misogynistic, and hypocritical, all characteristics Derfler describes but marginalizes. And perhaps worse, Lafargue evidenced few of the intellectual gifts that might have caused Benoit Malon, for example, or Paul Brousse (or even Jules Guesde himself, who never showed much genuine liking for his comrade) to forgive him his personal sins.

Derfler's is a biography worth reading, however, although it must be read with some balancing skepticism and supplementary material lest this version of events be accepted uncritically.

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SUSAN MILNER. *The Dilemmas of Internationalism: French Syndicalism and the International Labour Movement, 1900–1914*. New York: Berg; distributed by St. Martin's. 1990. Pp. viii, 260.

Susan Milner has written the first scholarly account of the origins of the International Federation of Trade Unions, which was called the International Secretariat of National Trade Union Centers before 1913. She has framed her study around conceptions of labor internationalism and efforts to prevent war that pitted revolutionary French syndicalists against a majority of nationally oriented reformists led by Carl Legien of the German Free Trade Unions. Labor internationalism was not just a spontaneous outgrowth of working-class consciousness, as was claimed in contemporary pronouncements, but rather a sentiment channeled and constructed by national trade union centers. By focusing on ideology and institutions Milner is able to offer us new insights into the collapse of proletarian internationalism before World War I.

While social historians have tried to explain the collapse as a result of working-class integration into the capitalist state through democracy, education, and social reform—a process hardly begun before World War I—Milner stresses the importance of ideology and leadership, especially that of Legien, who emerges from her account as the Samuel Gompers of European labor. Legien did everything as leader of the International to foster and propagate a nationally oriented, depoliticized reformist model of trade unionism similar to that advocated by Gompers, to limit the purview of the International to exchanges of information and strike appeals, and to protect its members against the dangerous radicalism of the French.

As revolutionaries against the capitalist state, the French syndicalists were the only members actively discussing and rehearsing the general strike that was supposed to occur in the event of war. Defeated in their attempts at a general strike by the French state and marginalized by the German leadership in the International, the French were persuaded to moderate their intransigence: to give greater attention to organization and reforms and to cooperate with French Socialists in efforts to prevent war. The clash with Legien's legions over the general strike made them wary of taking any unilateral action in August 1914 that would further the cause of German imperialism and threaten the democratic rights that they held dear. Notwithstanding some prominent ideologues like Emile Pouget, who dropped out of the *Conféderation Générale du Travail* (C.G.T.) in 1908, French syndicalists were more practical and republican than Milner allows in her severe conclusion. Once the syndicalists came out in support of a defensive war, a majority were led into collaboration with the state and into social reformism. This changed them from being revolutionary pariahs into the leaders of the new reformist International Federation of Trade Unions after the war.

Milner's book is quite properly an institutional and not a social history, but she should have given more attention to ideological diversity within each national movement. Milner recognizes that the French revolutionaries had to contend with a strong minority of reformist-minded trade unions in the C.G.T., but she tends to treat—much as the French did themselves—the German Free Trade Unions as a monolithic bloc. While radical labor activism was on the wane in France before the war, it was growing in many other countries, including Germany. Because of their organizational weakness and separation from the Socialists, however, the French were unable to provide revolutionary leadership internationally. That fell to the Russians after October 1917.

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LAURA LEVINE FRADER. *Peasants and Protest: Agricultural Workers, Politics, and Unions in the Aude, 1850–1914*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xiv, 260. $38.00.

This interesting sociopolitical history of viticultural capitalism in the French département of Aude is anything but parochial in its approach. Focusing on the vineyard village of Coursan, Laura Levine Frader never loses sight of the entire Audois vineyard region, while comparing this viticultural area with the rest of the Mediterranean Midi. Frader is equally familiar with and aware of work on the rise of capitalism in other settings.

This story of viticultural capitalism in Aude is above all a success story that turned sour. Taking advantage of a rising national demand for table wine, eastern Aude, led by great landed estates, switched from growing grain to tending vines. While these "wine factories" came closest to "pure" agrarian capitalism, small proprietors also turned to market viticulture and viniculture, which assured the region's prosperity from the 1850s to the 1880s. A lasting by-product of this "golden age of the vine" was the formation of a class of skilled vineyard workers, most of them also miniproprietors. This golden age collapsed under a series of shocks ushered in by the phylloxera blight, followed by the disruptive efforts to replant, which, ironically, led to subsequent crises of overproduction.

To this unique experience Frader ascribes the wine country's penchant for left-wing politics and, after 1900, the vineyard workers' attraction to syndicalist unions. Along the way she also pauses to analyze women's roles, both as unequal workers in the vineyard and as indispensable contributors to the family economy. The sociopolitical climax is the agrarian explosion of 1907 in which viticultural solidarity overrode antagonism between workers and employers.

Despite its extensively documented narrative, this study inspires some misgivings. Frader seems to lack a firm, down-to-earth grasp of how this society functioned. We are told, for example, of "the continued spread of landownership among all social and occupational groups" (p. 27), which raises the obvious question, if everybody bought land, who sold? Elsewhere (p. 69) she asks, "why did the *grande bourgeoisie viticole* allow them [the small proprietors] to survive?" She answers (without benefit of documentation) by listing the preservation of social stability, the keeping of a "reserve army of labor" and the maintenance of potential political allies. Yet it is doubtful that in the real world individual estate owners (there was never a collective decision) ever thought in such terms.

Somewhat related is the author's eagerness to apply social history concepts that prove less than illuminating in the context of the Aude vineyards. Is an understanding of twentieth-century vineyard strikers really enhanced by a disquisition (pp. 122–23) on E. P. Thompson's "moral economy of the poor?" Is "proletarianization" (pp. 70–77) an apt description of the evolution of a labor force that, despite declining living standards, remained highly skilled, largely landowning, and that, when confronted in 1907, rallied all wine growers against the state?

I am also disturbed by an uncritical use of dubious statistics to the neglect of more substantial statistical underpinnings. One single flagrant example suffices: to rely for a socioeconomic evolution of the viticultural population (p. 153) on either the occupational identification on land registers (p. 18) or on the census name lists (p. 29) shows unwarranted credulity. Checking the census lists for given years against the corresponding landholdings would have been more promising. In fact, Frader never provides solid statistics on changing property distribution in "her" village of Coursan, which routine cadastral analyses would have permitted.

Finally, the book displays gratuitous inconsistencies. To read on one page a harrowing description of misery in the Audois vineyards (dated 1882) and on the next the author's assertion that 1878 to 1884 was the acme of prosperity does not build confidence (pp. 61–62). To be told that, "unable to meet the high cost of treating and reconstituting the vineyards, small owners sold them" (p. 91), when this can be flatly contradicted by referring to table 5 (p. 25), baffles the reader.

Such shortcomings raise doubts about some of the author's interpretations. Nevertheless, this book provides an instructive case history of agrarian capitalism of interest to anyone concerned with French social history, peasant studies, and rural modernization.

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JO BURR MARGADANT. *Madame le Professeur: Women Educators in the Third Republic*. Princeton: Princeton University Press. 1990. Pp. xiii, 358. Cloth $49.50, paper $16.95.

Biography in a new key—life stories of individuals or groups of individuals as the basis of the examination of historical questions—is becoming prominent once again in the historical repertoire. Jo Burr Margadant's prosopography of the first generation of women professors in France illustrates the richness and appeal of such an approach.

Her analysis of the historical creation of that characteristic female profession—teaching—is based on information on just over 200 lives. She was able to find official files and supplementary materials about 80 percent of the women who comprised the first ten entering classes (1881–90) of the Normal School at Sèvres, the pioneering institution that trained many of the women who taught in the girls' lycées of Third Republic France.

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The book is organized around the life cycle of these women, following their collective fate from their beginnings as Sèvriennes in the 1880s, through their first posts as teachers in the provinces in the 1890s, their career development in prewar France, and their retirement in the 1920s. Throughout the analysis we see their experiences as a cohort shaped by the times in which they lived.

The entry of the first Sèvriennes into the teaching profession in the 1890s was marked by the novelty in that era of advanced education for women, producing in them a kind of heroic stance that they as a generation never really abandoned. If the decision to become a professional became more routine during the lifetimes of these women, in their youth it was anything but that. The particular dilemma of their formative years was to be "professional" and "feminine" as well. The crusading esprit of female educators in this epoch was also rooted in the emphasis placed by leaders of the Third Republic on the necessity for women educators to fulfill the political mission of educating girls for the state. The explicit competition with the Catholic church put pressure on this generation of educators to transform themselves into secular but morally impeccable mother figures.

At the same time, however, these women were often separated from their families and unlikely to marry, which meant that they needed to pioneer alternative living arrangements. Moreover, the drive toward professionalization set into motion intellectual and career aspirations quite distinct from the domestic models of womanhood previously exalted for middle-class girls. The tension between femininity and professionalization would remain with this cohort throughout their professional lives.

The mid-career years of the first Sèvriennes were marked by the increasing acceptability of education for middle-class women and by the political uses of educational expansion made by the state. Beyond the aims of state authorities, however, teachers found themselves subject to the rising expectations and demands of pupils and their parents. Margadant's analysis of evidence suggesting ways in which parental demand shaped the curriculum in the belle epoch represents an important advance over studies of educational reform that rest too heavily on administrative initiatives as explanation for change. Some aspects of state educational planning clearly failed: the highest status families never entrusted their daughters to state institutions and, conversely, an increasing number of lower-middle-class aspirants were drawn to the girls' schools with their promise of professional success. It was this "out-of-place" ambition that in part drove the system of girls' schooling toward rough equality with the separate boys' system.

As their careers developed, some Sèvriennes found a relatively settled life, and professional fulfillment, as *directrices* of provincial institutions. In establishing their schools as moral communities on the model of the school that trained them, *directrices* were arguably more successful than their much-criticized male counterparts. For those with the ambition to achieve posts in the more prestigious schools in or near the capital, however, frequent moves and a more disrupted personal life was generally the cost. But on the eve of World War I, the Sèvriennes could look with satisfaction at their collective accomplishments.

Their late careers were marked indelibly by the war and its reshaping of the female role. Domestic enshrinement was no longer sufficient. The war gave rise to a well-defined civic role for women educators even as it brought many women into public and male roles. The war and postwar era also pushed women educators away from a mode of self-presentation, which emphasized their unique and distinctive role as women, toward a claim that, although women's education remained separate, no one wanted it to be inferior or the conditions of women teachers to be less propitious than those of their male counterparts. The wartime reality and rhetorical shift, in other words, brought the claims of professionalization into a more prominent position than those of femininity.

Even though Sèvriennes played a major role in this shift, which was part and parcel of the longer process of female professionalization, Margadant suggests that many of them found themselves uncomfortable with it in their late career and retirement years. Their retirement years were in some cases marred by the increasing discomfort of the generation of pioneers with the "new women" of the 1920s. The claims represented both an extension and a rejection of the pioneering Sèvrienne model of heroic femininity and was hard for many of the older teachers and *directrices* to adopt.

The generation's life story thus ends on a slightly tragic note (perhaps the fate of all generations of pioneers?). These educators initiated and saw important transformations in women's lives and opportunities, only to find change passing them by. The reconstruction of their lives, in Margadant's skillful hands, allows us to witness with them the dramatic changes that they helped to bring about, but that they of course could not control.

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PETER BURKE. *The French Historical Revolution: The Annales School, 1929–89*. (Key Contemporary Thinkers.) Stanford: Stanford University Press. 1990. Pp. vi, 152. Cloth $27.50, paper $9.95.

Peter Burke defines the orientations of the directors and associates of *Annales d'histoire économique et sociale* (since 1946, *Annales: Économies, Sociétés, Civilisations*) as "the substitution of a problem-oriented analytical history for a traditional narrative of events" (p. 2), the observation of "the whole range of human activities in the place of a mainly political history" (p. 2), and

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collaboration with other disciplines in order to realize the first two goals. He breaks down the *Annales* movement into three phases, each embraced by a different "generation." Led by geography-oriented seiziemist Lucien Febvre and medievalist Marc Bloch, the first phase was a time of combat "against traditional history, political history, and the history of events." Fernand Braudel then virtually transformed the movement into "a 'school' with distinctive concepts." Since 1968, however, the movement has been "marked by fragmentation" (p. 2).

One aim of the author is "to explain the French to the English-speaking world" (p. 3), which had been cool toward Febvre's "expansive, vehement and combative" (p. 12) mood and his commitment to possibilist geography and *outillage mental*. Burke associates Braudel with the Spenglerian notion of civilization as routine and with Ratzelian geographic determinism and is critical of him for his neglect of attitudes and values and for not seeing "structures as enabling as well as constraining" (p. 40). Burke observes that "relations between Febvre's two 'sons,' Braudel and [historian of *mentalités* Robert] Mandrou, became less and less fraternal" (p. 43), resulting in Mandrou's resignation in 1962 as organizing secretary of the review. The overt breach had roots in the disappearance after Febvre's death of "a file of notes for an unwritten book which would have continued the study of Rabelais by considering the rise of the modern [pre–1789] French mentality" (p. 70), Mandrou's specialty. Braudel himself described the item, however, as "Lucien Febvre's *lost* book, *Honneur et patrie* (Honour and Fatherland), the revised version of the lectures he gave at the Collège de France in 1946 and 1947, and of which I actually held the manuscript in my hands in August 1956" (Fernand Braudel, *The Identity of France*, vol. 1, *History and Environment* [1988], 16). Mere whimsy does not explain the inclusion of the word "lost" in italics.

The schism between Braudel and the "third generation" came during 1968 and 1969, in the wake of the student insurrection of May 1968, culminating in the accession of younger scholars to positions of authority. *Annales* scholars subsequently became increasingly dependent on "American intellectual trends" (p. 66). In discussing Braudel's unfinished posthumous book, *L'Identité de la France*, Burke nonetheless overlooks the possibility that the choice of title may reflect France's own identity crisis and quest since the 1960s for economic growth. He appropriately concludes, however, that the *Annales* movement is "effectively over," having dissolved "in part as a result of its success." Looking at the *Annales* "in global perspective," however, one must "assess it as a paradigm." The uses and limitations of its methods may be uneven, varying "geographically, chronologically, and thematically" (p. 107), but it will not go away.

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DOMINIQUE ROSSIGNOL. *Histoire de la propagande en France de 1940 à 1944: L'utopie Pétain.* (Politique d'aujourd'hui.) Paris: Presses Universitaires de France. 1991. Pp. viii, 351. 175 fr.

RITA THALMANN. *La mise au pas: Idéologie et stratégie sécuritaire dans la France occupée.* Paris: Fayard. 1991. Pp. 394. 140 fr.

The "Vichy syndrome," as Henry Rousso calls the ongoing debate over the occupation years in France, has been and continues to be distorted by the fragmentary nature of surviving source material. Vichy sympathizers and Nazi collaborators, not knowing how things would turn out, disguised their intentions during the war and subsequently destroyed files. Postwar accounts are often apologetics. Two new attempts to sort things out are Dominique Rossignol's *Histoire de la propagande en France de 1940 à 1944: L'utopie Pétain*, which uses a variety of propaganda office files in the *Archives Nationales* for the period, and Rita Thalmann's *La mise au pas: Idéologie et stratégie sécuritaire dans la France occupée*, which relies largely on the files of the German Military Command in Paris (*Militärbefehlshaber in Frankreich* [*MBF*]).

Rossignol's subtitle, "*l'utopie Pétain*," posits a claim that even the Marshal and his supporters never made. Covering only 1940 through 1944, Rossignol removes the Vichy government from its historical context in the war that began in 1939. Thalmann also breaks sharply with historical continuity, assuming in 1940 a sudden *mise au pas*, which can mean a disciplining or a bringing to heel as with a dog. Much of the genesis for the authoritarian Vichy model, however, was set in Benito Mussolini's Italy, Antonio de Oliveira Salazar's Portugal, and the Spain to which Marshal Pétain had been ambassador in 1939. The Vichy *mise au pas* never was put fully into place in the sense of subordinating France to its Nazi masters. For example, Pétain refused to turn the French fleet over to the Germans. As a new departure in 1940, "*mise au pas*," in the sense of providing a firmer discipline for defeated France, could refer to the Resistance as well as to Vichy ideals. Thalmann, however, identifies the *mise au pas* with the Nazi *Gleichschaltung*, Hitler's political coordination or suppression of opposition in Germany (p. 36). *Gleichschaltung*, however, related only to German policy for domestic public institutions, not international relations, as Thalmann suggests in the case of Vichy France. Following Eberhard Jäckel's *Frankreich in Hitlers Europa* (1966), she shows how from the onset of the occupation Hitler wanted only to keep France pacified and exploit its economy for the German war effort. Even would-be collaborations were kept divided and used mainly as threats to keep Pétain in line (Thalmann, pp. 225, 267). Rather than a *mise au pas*, most of the French adopted a neutrality they hoped would spare them the worst effects of the war, and Thalmann herself points to Vichy's stalling in meeting German demands (p. 45, 94–95).

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Vichy propaganda imagery, painstakingly described and richly illustrated with archival photographs by Rossignol, relates to longer-term notions of imperial sovereignty of France and its empire, including in the interwar years the problems of Alsace-Lorraine, Italian claims to Nice and Corsica, Breton separatism, the Francophone zones of Belgium and possibly Switzerland, and overseas areas such as French Canada. Questions of capital-worker relations (the Popular Front, for example), aristocratic privilege, Spanish Civil War refugees, and the influx of refugee Jews, complicated propagandistic argumentation, as did failures of the parliamentary process, the rise of Nazi Germany, Bolshevism, and the desire to avoid a repetition of the bloodletting of World War I. Propaganda on all sides was surely no less intense during the *drôle de guerre*, the May 1940 campaign, and the emergence of the *Pétainistes* in June, than after the creation of Vichy when Rossignol's account begins. Rather than examine all propaganda between 1940 and 1944, as her title suggests, Rossignol focuses on three sets of iconography: Vichy's Pétain myth and National Revolution, the collaborationists' "European France," and the Germans' designated enemies, Leftists, Jews, Freemasons, Communists, and Anglo-Americans (p. 2). Only occasionally does she mention Allied, Resistance, church, or Italian propaganda in France. The middle phases of the war produced *la petite propagande* of everyday gestures, such as the installation of a new telephonic alphabet which changed, for example, "L as London" to "L as Lucie" (p. 211). Rossignol ends her account with the Liberation. The reentry into battle against the Germans, however, the purge trials, and the failure to fully reestablish governmental legitimacy until the creation of the Fourth Republic in 1947 are all part of the propaganda story.

Rossignol also makes unprovable claims, such as a "psychological terror" that she argues Vichy launched in the first half of 1941 (p. 21), and folkloric claims such as: "The French have a taste for medals" (p. 99). Her qualification of Vichy's propaganda as "totalitarian" (pp. 43, 81, 339) is vitiated by an emphasis in Vichy on the family as "the essential cell" of the social order (p. 141), and her own reference to the Portugal of Salazar as a model for Vichy corporatism (p. 158). The staging of the exposition "Jew and Mason" in Marseille in February 1943 showed that even Pétain was no longer master of his own propaganda (p. 253), hardly the recipe for a totalitarian state. Although Vichy propaganda is described as technically impressive (p. 266), Rossignol offers little evaluation of its effectiveness.

A fuller assessment of what German observers saw as the failure of Vichy, collaborationist, and their own propaganda is offered in Thalmann's *MBF* reports, which by September 1940 showed a widespread distrust of Pierre Laval and the Vichy administration, even if Pétain was much admired (p. 75). Thalmann points out that the Germans never used more than 40,000 men to occupy France (p. 23), and, as Robert Paxton has argued (*Vichy France* [1972]), they could successfully carry out Hitler's agenda only with the help of French collaborators. The Germans did not force the French to change their form of government in July 1940; indeed, Hitler was indifferent to the National Revolution (pp. 268–69, 271). Given Vichy's inability to get the Germans even to allow the raising of the tricolor or the playing of the *Marseillaise* in the occupied territories (p. 273) and its anticipation of German demands in deporting Jews, Thalmann concludes that its "connivance with the occupier singularly aggravated the situation and anesthetized the conscience of the greater part of the French" (p. 274), alas, an assertion impossible to prove. Thalmann sees public opinion turning against Vichy in the second half of 1941 when Admiral François Darlan failed to secure a peace treaty of cooperation with Germany and when the invasion of the Soviet Union began a cycle of Resistance attacks and German reprisals, in which Pétain saw "an evil wind" turning the French against him (p. 290).

Both of these books offer useful new information on Vichy France. The authors, however, fail to carry forward their own titular programs to logical conclusions and neither book is organized chronologically. Instead we find unprovable arguments, such as Thalmann suggesting that xenophobia and the rise of the National Front in France were caused by a glut of information in the contemporary world (pp. 9–10) or Rossignol asserting that an abundance of iconography so deeply rooted the Pétain myth in France's collective unconscious that descendants forty years later have difficulty getting rid of the image and have perpetuated Rousso's "Vichy syndrome" (p. 15).

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PIERRE GERBET. *Le Relèvement 1944–1949*. Assisted by JEAN LALOY *et al.* (Politique étrangère de la France.) Paris: Imprimerie Nationale. 1991. Pp. 481.

The third volume in a distinguished series directed by Jean-Baptiste Duroselle, this book traces French foreign policy from 1944 to 1949. Following the pattern of the two preceding volumes, *La Décadence (1932–1939)* and *L'Abîme 1939–1944*, Pierre Gerbet, with the collaboration of eminent diplomats Jean Laloy, Jacques Dupuy, Jacques de Folin, and Henry Rollet, has produced a masterful portrait of French policy during a traumatic period of transition in world politics.

The first seven chapters analyze French responses to a series of major challenges to traditional national interests from the closing days of World War II until 1947. Operating in a new setting dominated by the Big Three powers, French leaders strove in vain to restore France to an active role in European and global politics. General Charles de Gaulle and virtu-

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ally all the leaders of the major political groups, however, conceived of that role in traditional ways that led them to adopt policies having little or no chance of success. A punitive policy toward Germany found no support among the major allies, and the effort to rebuild France's eastern policy around a Franco-Soviet alliance was no more successful. Ultimately, economic weakness forced France to seek American financial aid, despite de Gaulle's determination to remain independent. His desire to restore France's "rank" among the major world powers led to a misunderstanding of the nature and strength of colonial nationalism, to a hopeless struggle in Indochina, and to a series of unsatisfactory compromises on colonial issues. These failures set the stage for later conflicts in North Africa and the eastern Mediterranean that undermined the legitimacy of the Fourth Republic's constitution.

The second part of this book examines France's shift away from a policy of non-alignment between the emerging superpowers to one of active participation in the Western bloc. The change was not easy. Gerbet offers a particularly shrewd assessment of the difficulty of policy making under the new institutions of the Fourth Republic as new policy orientations were strongly opposed both by the Communist Party and by many followers of de Gaulle, as well as by others whose idea of French interests derived from an earlier epoch when both power and influence were more abundant.

The authors' judgments of the actions and policies are judicious and give full weight to the evidence that has come to light in the last two decades in memoirs, interviews, and in studies sponsored by the Institut Charles de Gaulle. In making their assessments, however, they also take full account of the turmoil and confusion that surrounded these events and the consequences of actions of minor officials or inactions of prominent leaders. American readers will take particular satisfaction from the demolition of some Gaullist myths, such as the division of Europe at Yalta or the American responsibility for French failure to regain Indochina. The authors have produced a highly readable study, written in a lucid style with enough citations and references to provide specialists with a clear indication of their sources but not so many as to deter a general reader.

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Diane Rubenstein. *What's Left? The École Normale Supérieure and the Right.* (Rhetoric of the Human Sciences.) Madison: University of Wisconsin Press. 1990. Pp. xv, 215. Cloth $40.00, paper $19.75.

There is, Diane Rubenstein tells us, a "leftist caricature" of the École Normale Supérieure (ENS) that focuses only on the brilliant left-wing politicians from Jean Jaurès to Léon Blum who were alumni of the school (pp. 9, 14, 54). But the ENS also produced a significant number of fascist intellectuals, like Robert Brasillach, Marcel Déat, and Thierry Maulnier, who usually preferred journalism to party politics. Nor, she argues, should this be altogether surprising, because the "institutional discourse" of the ENS, properly decoded, demonstrates powerful affinities with the ideologies of the Right (pp. 9, 68). By decoding the ritual and language of the ENS, Rubenstein attempts to show how, its leftist pretensions notwithstanding, it was an ideal "nursery" for right-wing intellectuals. She also presents a particularly effective analysis of the literary networks of the 1930s and a brilliant chapter on the postwar collaboration trials. To be sure, some of this ground has been previously covered in Robert J. Smith's *The École Normale Supérieure and the Third Republic* (1982), unaccountably absent from Rubenstein's notes and bibliography. But Rubenstein approaches the topic from a radically different perspective.

Rubenstein addresses herself primarily to the devotees of deconstruction. This is a work replete with *Aufhebungs* and tropes. The uninitiated are rarely enlightened as to the meaning of this technical vocabulary, although for some reason they are repeatedly given the original French for such seemingly unproblematic words as "dream," "pot," "leader," and "fashion." The crisp discussions about the ENS are marred by the endless and tedious digressions rehearsing all the arcane literary controversies of Jacques Derrida, Michel Foucault, and Pierre Bourdieu.

Rubenstein clearly believes that this cumbersome theoretical apparatus is essential for an understanding of her subject. This is not obvious. She is surely wrong to contend that "traditional political science . . . is woefully inadequate" to account for the preference of the right-wing *normaliens* for journalism rather than elective politics (p. 28). Anyone familiar with the drab political culture of the interwar parliamentary Right can understand why it would hold little appeal for a Maulnier. Nor are the author's own ruminations about the significance of the homonyms "right" and "write" especially illuminating (p. 15).

Rubenstein's preoccupation with teasing out the deeper significance of words, moreover, can lead to very strained arguments. The impeccably democratic and civil-libertarian director of ENS, Célestin Bouglé, believed in the importance of physical education. So did Abel Bonnard, Minister of Education under Vichy. For Rubenstein this suggests a common "fascist discourse" (p. 66). What it really suggests is that concern for physical fitness is politically neutral. Indeed, there seem to be discrete limits to the explanatory power of "institutional discourse." The discourse of the ENS may have been, as she insists, "consonant" with "fascist thematics" (p. 68). But the majority of students exposed to it remained left-wing republicans, so presumably that discourse is also consonant with liberal democracy. The rites and rituals of the ENS did not insulate its students from

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the seductions of fascism but they probably did not convert anyone to fascism either.

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JONATHAN BROWN. *The Golden Age of Painting in Spain.* New Haven: Yale University Press. 1991. Pp. ix, 330; 290 plates. $65.00.

In this stimulating and beautifully produced book, Jonathan Brown defines the milieu of Spanish painting in its fullest possible sense, rather than analyzing art and its practitioners as somehow divorced from the world around them. He rejects the assumption that a deeply embedded national identity shapes the production of a nation's artists, instead treating Spanish art as the product of diverse traditions both inside and outside Spain. He also includes artists such as Titian, who never visited Spain but nonetheless produced his most important works for the Spanish Habsburgs, and Rubens, born in the Spanish Netherlands and patronized by the Spanish elite. Brown emphasizes the importance of artistic influences from Italy and Flanders and their adaptation to the Spanish market. His key point is that the cultural diffusion of style needs to be given a respected place in art history, which has traditionally valued originality and treated borrowing as proof of second-rate status.

Brown's analysis groups artists and artistic developments from 1474 to 1700 into broad stylistic periods, devoting separate chapters only to El Greco and Ribera (El Greco because he lacked royal favor, and Ribera because he chose to work in Spanish Naples rather than in Spain itself). In the late fifteenth century Flemish and Italian models redefined Spanish painting, and in the early sixteenth century the Emperor Charles V and his siblings established Titian as an abiding artistic force in Spain. Philip II (1556–98) served his political and artistic apprenticeship in the widespread domains of his father the emperor. Even before he became king of Spain, Philip was a knowledgeable patron of art, and he used the crown's power to impose Italian and Flemish styles on Spanish artists from his newly designated capital at Madrid. The only blot on his record as a connoisseur was his lack of appreciation for El Greco, who nonetheless flourished in Toledo with the support of influential ecclesiastics.

Philip III (1598–1621) and other elite patrons favored realistic paintings inspired by Tuscan naturalism, and religious patrons made sure that a rigid Catholic orthodoxy prevailed in the work they commissioned. Philip IV (1621–65), a great collector like his grandfather, established the work of Rubens in Spanish taste. During his reign Velazquez, Ribera, Zurbarán, Murillo, and other giants of Spanish painting flourished as well. Brown makes a persuasive case that buyers' tastes shaped painting in "golden-age" Spain. Overall, the elite patronage network tended to favor foreign models and to hold Spanish artists in low esteem, which Ribera used to explain his residence in Naples. Spanish painters made the best of the situation by adapting to the market, controlling the training of apprentices, and fostering their own artistic dynasties.

Brown's analysis is a successful melding of art and history rather than a narrowly focused history of art. A few lapses of historical fact and interpretation are worth mentioning: Philip the Fair and Juana of Castile had six children, not four (p. 36); and the economic revival of the late seventeenth century was much more widespread and important than Brown realizes (p. 285). These errors hardly detract from the enormous value of the book, however. Brown defines Spanish painting in a context broad enough to reflect the vast and complex character of the Habsburg dominions.

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WILLLAM MONTER. *Frontiers of Heresy: The Spanish Inquisition from the Basque Lands to Sicily.* (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1990. Pp. xiv, 345. $39.50.

The subject matter of this book is more specific than its broad title suggests: William Monter in fact studies all the business of the non-Castilian part of the Spanish Inquisition in Europe, an area that he refers to as the "Aragonese" secretariat (although "Aragonese" is a somewhat inconvenient term when referring to realms as diverse as Navarre, Sicily, and Catalonia). He takes in the first 300 years of the "Aragonese" tribunals, basing himself mainly on the source known as the "*causas de fe.*" Coming to the subject without a specialist training in Iberian history, Monter manages to achieve a superb focus on issues that have usually escaped the attention of scholars apparently more familiar with the field. The bulk of existing studies on the Inquisition gives excessive weight to its Castilian character; Monter by contrast gives a learned and detailed exposition which shows that things were very different outside Castile. He concludes, for example, that from the late sixteenth to the early seventeenth century it was the non-Castilian tribunals that were the most active, accounting for over half of recorded executions, mainly in spheres where the Castilian Inquisition was virtually inactive, in offenses such as sodomy (on which Monter gives the best account available) and witchcraft. He also argues that in the non-Castilian realms the Inquisition was used to "provide valuable prestige for Castilian government." Apart from giving an excellent survey of inquisitorial activity in the various provinces, he contributes original and important data on the persecution of Moriscos in the kingdom of Aragon.

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This is the most stimulating book on the Inquisition to appear in twenty years. Where most specialist scholars have restricted their vision to a single province of Spain, Monter extends his attention over the western Mediterranean and comes up with refreshing perspectives that will hopefully change our way of looking at the Inquisition. As with the original classic work by Henry Charles Lea, Monter places the tribunal firmly within its political context and thereby makes much sense of some of its history. His treatment of Catalonia is perhaps one of his few weak points: it is unwise to rely on the Venetian ambassador for an opinion of royal policy over the Inquisition in Catalonia, the people of the Val d'Arán were Catalan (Aranés) and not Gascon in speech, the incompatibility of familiars for public office dated not from 1599 but from at least 1553 and definitively from 1585, and far from being ignored by the rebellious Catalans in 1640 the Inquisition was one of the first Castilian institutions to be attacked and ransacked. Although one may quarrel with some detail in a book that covers so much ground, there is every reason to be grateful to the author for his splendid, original, and stimulating study, a major work that considerably enriches our understanding of the Spanish Inquisition.

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ANTON M. PAZOS. *El Clero navarro (1900–1936): Origen social, procedencia geográfica y formación sacerdotal.* (Colección história de la iglesia, number 17.) Pamplona: Ediciones Universidad de Navarra. 1990. Pp. 503.

Anton M. Pazos attempts to present a comprehensive view of Navarrese clergy from the moment they enter the seminary until their ordination and appointment to parishes. The author stresses the ultra-traditionalist nature of Catholicism in Navarre at the turn of the century and claims to explain the pressures brought to bear on the education and training of the clergy by contemporary events. The outbreak of the Civil War in 1936 and its effects on the clergy in Navarre are rather disappointingly outside the limits chosen for this study. In asserting that many of the people from Navarre have an innate sense of religion (80 percent of the people attending Mass between 1900 and 1930) and that, to a great degree, this accounts for the high number of young men who entered the priesthood before 1936, Pazos fails to show how this differs from other areas, if indeed it does, as no comparative material is included. The diagrams and statistical analyses indicate the areas from which the clergy were drawn, the professions or occupations of their families, and the numbers of ordained men per 10,000 inhabitants (chapter 8) but, while this is apparently a feature of Navarrese society, it is what historians of the Spanish church would expect and therefore is in no need of elaboration.

Other aspects of clerical life in Navarre examined here concern education, reforms, and instability and the less-than-ideal living conditions of seminarians. The use of manuals and memorizations were proven methods, essential to the efficacy of the theological training given. Because students came from traditional Catholic families they tended to be uncritical in their approach and it was only when reforms were attempted and failed that a critical period in Navarrese clerical training was evident. For instance, the furor over reform removed Legaz, who had been rector for thirty-six years, in 1902; and it was not until 1910 that the administration of the seminary recovered some measure of stability. Through this period of crisis, which coincided with a troubled period in Spain's history, the reader cannot help wondering what repercussions the social unrest in much of Spain from 1909 to 1936 had on Navarre. The author makes little attempt to relate the position of the Navarrese clergy to the situation of the clergy in Spain as a whole.

Finally, the inclusion of a plethora of details and too many quotations from secondary works, many of which could have been relegated to notes or put in an appendix, make reading wearisome. Long quotations frequently detract from the essential theme of the book and in most cases the ideas would have been better presented in the author's own words.

This book should be regarded more as a reference work than as a study of the significance of the clergy in Navarre. The statistical data, the maps and diagrams, the use of archival sources, and the extensive research done by the author all provide material that will be invaluable to scholars of the Spanish church. Pazos has not come up with any new conclusions on the church in Navarre but the conclusive data he supplies prove beyond doubt the long-established view of the traditional catholicity of both clergy and people.

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NEIL MACMASTER. *Spanish Fighters: An Oral History of Civil War and Exile.* New York: St. Martin's. 1990. Pp. ix, 250. $45.00.

Fifty years of historical scholarship on the Spanish Civil War have produced an important corpus of work: detailed comprehensive narratives of political and military developments together with a growing number of monographs and memoirs intended not to justify but to explain individual participation in the war. What would seem to be most needed at this point, after the collapse of European Communism in 1989, is a new informing scholarly vision both of the Loyalist cause in particular and of that national and

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international conflict in general. By setting to one side the political terms that have until now framed the discussion, social history particularly offers a new means to link the experiences of individuals and of groups with the larger forces that determined events (military, economic, and political aggression; social justice; a search for identity in the modern world).

Ronald Fraser began this new quest for an understanding of motivation and personal consciousness by both Loyalist and Nationalist participants (not leaders) in his now classic oral history, *Blood of Spain* (rev. ed., 1986). Neil MacMaster, a British historian, follows in this tradition of writing "history from below." Based on conversations recorded between 1979 and 1982, this book recounts the lives of David Granda and Consuelo Contreras from their childhood of poverty in Asturias, through their participation on the Republican side in the war, and into the long journey of exile (internment camps, forced labor, menial low-paying work as "guest workers" in the south of France). At the end, following their two recent unsuccessful attempts to return to live in Asturias, husband and wife record their profound sense of loss of identity and community. Defeated in war, aliens in France, excluded from Granda's extended family in Asturias and from the new political culture of post-Franco Spain, Granda concludes "we not only lost the war, we lost everything else too. Everything" (p. 288).

Unfortunately MacMaster's neatly crafted book does not transform this account by two articulate individuals into a serious work of social history. One problem is methodological: not oral history as such but its use. All that we know comes from Granda and Contreras; MacMaster interviewed neither their family nor their friends, let alone their antagonists. Still another problem is language. Did MacMaster talk with them in French or Spanish? (No book in Spanish is cited, and the few idioms quoted seem inept.) The equivalent is British, not American English: "sent to Coventry" (p. 188), "good old boy" (p. 67). Do these expressions bring us closer to the life experiences of Spanish workers? Does MacMaster's transcription of their experience mirror their feelings or instead his specialized research in migrant labor in France? One example: in the case of the ancient French-Spanish hostility, did Granda really use "racism" (p. 191) to describe its present format? Contreras, in contrast, recounts personal incidents of French hostility.

If, however, it is context that gives meaning, then the reader has only the relatively impoverished footnotes based on a very few English-language general histories to supply that context (MacMaster cites, but does not explore, Adrian Shubert's masterly *The Road to Revolution in Spain* [1987] on the very region where Contreras and Granda lived). The history of Asturian society in general, and the world of its workers, has dramatically expanded over the past decades. So has the bibliography on the Spanish Communist Party in the war and in exile. As active members until the 1950s of a relatively select and small exile party (perhaps totaling only 10,000 in about 1949) Contreras and Granda's account does not give us the valuable insight we might have expected from their rank-and-file perspective.

The collaborative work of scholar and participants might even be expected, for example, to provide understanding about universal concerns: the impact of ideas on human consciousness, of ideology on human behavior. Or at a more human level, of the need for community, be it through party membership or extended family. We can only speculate what, if asked, Granda and Contreras might have answered to these and other more mundane but significant questions.

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ROBERT S. DUPLESSIS. *Lille and the Dutch Revolt: Urban Stability in an Era of Revolution, 1500–1582*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. Pp. xv, 372. $54.50.

Robert S. DuPlessis has written an important monograph that will undoubtedly be cited frequently in the future by scholars working on practically all aspects of the Habsburg Netherlands in the sixteenth century. Lille was one of the few thriving textile towns of the Habsburg Netherlands in the period after 1500 and by the late sixteenth century was one of the half-dozen largest cities of the region. The object of his study is twofold. First, his intention is to provide a detailed, general portrait of a major town: its economy, civic institutions, social structure, welfare system, and religious propensities. Second, by examining the causes of the relative stability of Lille compared to the other main cities (or most of them) of the Netherlands, and the relative weakness of Protestantism there, he explores the general question of why some cities of the Low Countries during the Age of Revolt against Spain were much more prone to political and social instability, and to Protestant influence, than others.

The book is strongest in its first capacity, as a portrait of a particular town in the Age of Revolt. DuPlessis has amassed a formidable array of statistical and other data relating to the city's economy and social conditions, and he is particularly skillful in demonstrating the basic trends in wage, price, and work conditions and relating these to levels of protest and consensus in the attitude of different groups, and of the town's population as a whole, to the civic government and to central government, as well as to the Catholic church. The essence of his argument is that while Lille was subjected to largely the same pressures as most other towns (long-term erosion of living standards and a severe crisis of conditions in the mid–1560s), the lack of major change in the

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structure of civic society, work relationships, and the balance between social groups enabled the ruling patrician elite to consolidate its power and pursue energetic policies of social welfare and defense of the Catholic church, thus minimizing social tension and disaffection with the church. DuPlessis's tendency to explain religious and political attitudes in an almost mechanistic way in terms of levels of social stability and disaffection will, no doubt, arouse a good deal of debate, and in my own view the sections on religion and politics are not quite so robust and well-argued as the chapters on material life. Nevertheless, as a portrait of a town this is a monograph of unusually high quality.

One is bound to have more reservations about the book in its second capacity. Here DuPlessis is open to two main criticisms. First, he seems to be not particularly familiar with the literature in Dutch, and the picture that he gives of conditions in the Dutch-speaking parts of the southern Netherlands, and in the northern Netherlands, is not always accurate. Occasionally there are embarrassing slips, as when he refers to "Brill in Zeeland." But my chief objection is to the way that he depicts Lille as highly exceptional, almost an island of stability amid a sea of disaffection, insurrection, and Protestant agitation. In fact there were many towns in the Habsburg Netherlands, and especially in the south (although I would also include Groningen) where Catholic support was strong, Protestantism relatively weak, and levels of protest and opposition to civic and central government notably low. This is particularly true of the French-speaking Walloon areas that (contrary to the impression given by DuPlessis and most well-known textbooks on the Dutch Revolt) were, in the main, the most stable and the most Catholic part of the Habsburg Netherlands. A few Walloon towns, notably Tournai and Valenciennes, may have been major Protestant centers, and the focus of unrest and insurrection, but most of the Walloon region, including the towns of Arras, Mons, Namur, and Douai, as well as the independent bishopric of Liège, certainly was not.

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GARY K. WAITE. *David Joris and Dutch Anabaptism, 1524–1543*. Waterloo, Ontario: Wilfrid Laurier University Press. 1990. Pp. xi, 235. $32.50.

One of the most notorious—and creative—reformers of the Dutch Reformation was David Joris (1501–56). Gary K. Waite presents here an intellectual biography of Joris, placing him in the context of the Dutch Reformation. Although Joris defies easy classification into any one religious category, he chiefly belongs to the Anabaptists. Many Anabaptists of his own day did not care to claim him, however, and similarly many twentieth-century Mennonites do not much appreciate him. At various times he was a Sacramentarian, a Melchiorite, a fiery iconoclast, and at the end more of a Spiritualist.

After the Anabaptist catastrophe at Münster, 1534–35, the Dutch Anabaptists split into many competing factions, searching for light and hope. These included the Batenbergers, the Melchiorites, the Mennonites led by Menno Simons, and Joris's faction, the Davidites. For several years in the late 1530s, Joris was a leading contender for leadership of the Dutch Anabaptist movement. He lost out to Menno, who gave his name to the entire movement.

Waite describes and analyzes the distinctive beliefs of Joris. In the tradition of Melchior Hoffman of Strasbourg, Joris preached an apocalyptic kind of Anabaptism, spiced with visions and wonders. His message expanded beyond the words of Scripture, as he tapped inexhaustible spiritual resources: "the Word of God is not to be composed or comprehended with ink, letter or pen, but only through the Spirit" (p. 129). His spiritualist Anabaptism was innovative indeed, too adventuresome for Menno, his chief rival, who warned pious Christians against him. "Is it not a grievous error that you suffer yourselves to be so woefully seduced by such worthless persons . . . from Beelzebub to Lucifer, and from Belial to Behemoth?" (p. 151). Waite shows how Joris moved from belief to belief, and how these various positions relate to Anabaptism as a whole.

There were many unique influences on Joris's life, apart from his visions, such as the chambers of rhetoric at Delft in which his father was an actor. The name David (from King David) came from a role played by his father. Eventually, Joris imagined himself to be God's "third David" in the line of the Israelite David and Christ the Lord. Another key influence came from Anneken Jans. Waite credits her with being Joris's first teacher in the knowledge of "divine dreams" and "night visions" (p. 68). Toward the end he abandoned the Netherlands, moved to Basel, lived under an assumed name, and died there.

Waite's presentation is clear and the material well researched. The book is helpful for understanding not only Joris but also the larger issues of the Reformation. Waite makes it evident why the unpredictable Joris lost out in the leadership competition to steady and pious Menno, and at the same time why the prophetic Joris was so appealing. His books continued to be published and popular into the seventeenth century. Cornelis Claesz of Amsterdam, one of the biggest book dealers of the city, had his works for sale. The 1610 auction catalogue of his stock offered both Menno's *Foundation Book* and David's *Wonder Book*. The battle of the books went on.

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ANDREW C. FIX. *Prophecy and Reason: The Dutch Collegiants in the Early Enlightenment*. Princeton: Princeton University Press. 1991. Pp. xii, 277. $42.50.

Andrew C. Fix has written a useful contribution to our understanding of the complex transition in European intellectual life from the religious world of the Reformation to the secular world of the Enlightenment; from faith to reason. Much of the recent literature on the connection between religion on the one hand and modern science or rationalism on the other studies English Puritans and radicals. Fix's case study is a group of radical Dutch Protestants who, disillusioned with the Dutch Reformed church, began in 1620 to meet in informal "colleges" for prayer, Bible study, and mutual exhortation.

The Collegiants, as they came to be called, were among the more radical groups in the Dutch "Second Reformation." Among the colleges' founders were Dutch Arminians, Mennonites, Spiritualists, and Millennarians, later to be joined by Socinians and adherents to the thought of Descartes and Spinoza. Fix meticulously traces each of these varied roots, providing brief intellectual biographies of each important Collegiant and of each earlier thinker or movement that in turn influenced the Collegiants, thus producing a richly detailed sketch of the various strains of radical Protestantism in the Netherlands during the late sixteenth and early seventeenth century. The early Collegiants were convinced that Christ would soon usher in the millennium and restore the corrupt world in which they lived. They awaited his return. The churches, they believed, were equally corrupt. The Protestant churches were as devoid of the New Testament marks of the spirit and thus of the authority to preach and teach as was the Roman church against which they had rebelled. In the absence of the true church the best that sincere believers could do while awaiting Christ's return was to meet to study the scriptures and share each other's spiritual insights or prophecies. Lacking unambiguous divine authority, the Collegiants advocated tolerance of theological differences and respect for each member's scripturally based prophecy. Fix demonstrates how after 1660, with the millennium so long delayed, the Collegiants—influenced by Socinian and Cartesian members and by Spinoza himself—gradually replaced the individual believer's spiritual inner light as the guide to understanding scripture and to religious truth with the natural light of reason. Their reaction to Quaker extremism also drove them toward rationalism.

Fix sees Spinoza's rationalism at the end of the Collegiants' metamorphosis, although he draws back from actually calling Spinoza a Collegiant despite the philosopher's close relations with some Collegiant leaders. He also suggests a possible Collegiant influence on Spinoza's thought, alongside the Cartesian, Scholastic, Jewish, and Neoplatonic influences. He does not mention Pierre Bayle's suggestion of a Neoconfucian influence. The book's topical organization and Fix's thorough explanations of the background and antecedents to each part of the story results in some repetition and in places a sense of over-documentation. Nevertheless, the story of the Collegiants is probably in any telling a complicated piece of intellectual history, and it emerges with considerable clarity from Fix's hand.

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TOIVO NYGÅRD. *Suomen paluelusväki 1600-luvulla: Palkollisten määrä, työ, palkkaus ja suhteet isäntäväkeen* [Hired Labor in Seventeenth-Century Finland]. (Historiallisia Tutkimuksia, number 150.) Helsinki: Suomen Historiallinen Seura. 1989. Pp. 184.

Toivo Nygård's book is an example of the kind of work Nordic historians do very well: a professional, factual analysis of a strictly defined social problem. In this case the subject is farm servants, male and female, of seventeenth-century Finland. This sector of the laboring classes was distinct from the cottagers, squatters, and such other masterless or migrant groups as soldiers. Indeed, a major incentive for being a farm servant, for males at least, was that those in the masterless category were first in line to be selected for military service, a fate most sensible men were desperate to avoid. Farm servants were mostly unmarried and were bound by an annual contract.

Sweden-Finland, like most early modern societies, wanted its labor force under strict control; as it did not have serfdom, it had a legal code prescribing terms and conditions of service, made general in two labor laws of 1664 and 1686. The contract, which was legally binding, gave the servant a hiring fee, a cash wage, a package of clothing, and board and lodging. Servants could not refuse a legal offer of employment if he or she was currently without a master. They could leave their employer only during the prescribed hiring week in the autumn; if they absconded at other times they could be legally recovered by the master. Servants were subject to the full paternal authority of the employer during the period of service. As propertyless commoners, the servant class could not leave their parish of settlement without a permit from the priest. Although in law they were freemen and taxpayers, able to sue for their rights in the courts, the law was heavily loaded in favor of the masters.

Nygård begins by reviewing his sources, the two main categories of which are taxation rolls (after 1634 every adult person should have been registered for poll tax) and court records of master and servant disputes. He is well aware of the limitations and interpretive problems of his sources, but these kinds of materials are unusually complete and abundant for early-modern Sweden-Finland. Nygård demonstrates

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that while the servant class increased, perhaps four or five-fold during the century, it remained a modest proportion of the total population, ranging from 3 to 7 percent. In the more developed southwest of Finland, most households had at least one servant, in line with the European norms for the period, but in the more primitive east only one household in three kept a servant.

The increase in servant numbers, which is both absolute and relative, reflects a slowly expanding economy. Maximum wages were prescribed by law, but in most of Finland the law was not observed; there were marked variations dictated by local contingencies and supply and demand. It is difficult to determine what the conditions of work and standards of life for servants were. In principle the master had very wide legal authority, but the paucity of court cases arising from treatment or conditions of service suggests they were probably tolerable.

Nygård's book is very professional: he sticks to ascertainable fact, does not venture to bridge gaps in the evidence by hypothesis, and seeks to draw only the most limited conclusions from his evidence. His ideal appears to be the old Germanic ideal for historians, to tell it as it really was. He seems content to leave it to others to draw broader conclusions from his material. But within these limits it is a valuable contribution to knowledge about early modern European society. It has a summary in English and a good bibliography.

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JÜRGEN HABERMAS. *The New Conservatism: Cultural Criticism and the Historians' Debate*. Translated by SHIERRY WEBER NICHOLSEN. Foreword by RICHARD WOLIN. Cambridge: Massachusetts Institute of Technology Press. 1989. Pp. xxxv, 270. $22.50.

This volume consists of essays written by Jürgen Habermas in the 1980s in response to what he saw as the emergence of a new conservatism in the Federal Republic of Germany. Centering on German political culture, they form an important supplement to Habermas's philosophical writings of the decade, particularly his critical assessment of postmodernist thought in *The Philosophical Discourse of Modernity* (1987). As Richard Wolin notes in his introduction to this volume: "Since the early 1980s, Habermas has shown considerable interest in exploring the possible links between the politics of neoconservatism and the philosophical implications of what is known as postmodernism. In his view it is far from coincidental that what were perhaps the two most significant intellectual trends of the 1980s emerged and flourished concomitantly" (p. xxi).

All these essays center on the crucial question of the moral obligation for Germans today for the crimes against humanity committed in their name—the burden of Auschwitz—which makes traditional conceptions of national identity and history untenable. "After Auschwitz," Habermas writes, "our national self-consciousness can be derived only from the better traditions of our history, a history that is not unexamined but appropriated critically" (p. 234). In the past Germans have sought to separate themselves from the political and intellectual ethos of the West. The great achievement of the Federal Republic of Germany in the forty years of its existence, Habermas argues, has been that it "opened itself without reservation to the political culture of the West" abandoning the old exclusionary nationalism for a "constitutional patriotism" (p. 227). Germany's orientation to the West was established not only "pragmatically, as a matter of alliances, [but] intellectually as well, as a new beginning in political culture" (p. 266). This consensus, on which the official understanding of the Federal Republic has been based, has now been terminated by the Right (p. 234), for whom, to quote Franz Josef Strauss, "It is now high time for us to step out of the shadow of the Third Reich and the aura of Hitler and become a normal nation again" (pp. 246–47).

This is what in Habermas's view the *Historikerstreit* is all about. It was Habermas who opened the debate in *Die Zeit* in the summer of 1986, challenging Ernst Nolte's attempt to negate the unique responsibility of Germans for the Holocaust by presenting it as a reenactment of Stalin's mass killings and a defensive reaction against communism. Habermas as a nonhistorian was severely criticized, not only for taking issue with Nolte but also for contesting the views of Andreas Hillgruber and others who sought to rescue the honor of the German soldiers for defending their homeland in 1944–45 against the Soviet menace. In a similar spirit, Chancellor Helmut Kohl visited Bergen Belsen in the morning and the Bitburg military cemetery in the afternoon on the fortieth anniversary of the end of World War II. But, as Habermas argues, this is not "a debate about historical details. In reality, what is involved is the translation into politics of the revisionism appearing in contemporary historiography" (p. 229).

Other essays deal with the role of the intellectual in Germany. What Habermas bemoans is the emptiness of a political culture that had no room for critical intellectuals like Heinrich Heine and Kurt Tucholsky and that instead produced "nonpolitical" mandarins like Carl Schmitt and Martin Heidegger, to each of whom he devotes separate essays. Key to his examination of Heidegger is "whether there was an *internal* connection between Heidegger's philosophy and his political perception" (p. 145) which led him to Nazism. Very much as in his philosophic works, Habermas makes a plea in these essays for a reaffirmation of Enlightenment values of communicative reason, pluralism, and respect for human dignity, coupled with

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Walter Benjamin's warning that culture always bears within it the danger of barbarism (p. 252).

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OTTO ULBRICHT. *Kindsmord und Aufklärung in Deutschland.* (Ancien Régime, Aufklärung und Revolution, number 18.) Munich: R. Oldenbourg. 1990. Pp. 462. DM 140.

RICHARD VAN DÜLMEN. *Frauen vor Gericht: Kindsmord in der frühen Neuzeit.* Frankfurt a.M.: Fischer Taschenbuch. 1991. Pp. 155. DM 16.80.

In 1614 the authorities of Nuremberg executed a twenty-four-year-old servant for the crime of infanticide. Her story is typical. She had met a journeyman who, on the promise of marriage, slept with her and then disappeared. When her pregnancy became apparent her employers dismissed her. After searching arduously, she took a position with a female weaver. On a Friday night, unattended, she bore a child. In the trial testimony she admitted holding the baby in her arms, killing it, and throwing the body, wrapped in an apron, into a latrine. The court sentenced her to death by the sword.

Thousands of women during the early modern era in German-speaking Europe suffered similar fates. Only the crime of witchcraft surpassed infanticide in bringing women to execution in this period. Otto Ulbricht and Richard von Dülmen take different approaches in their studies of this widespread phenomenon.

Ulbricht places infanticide in the context of the Enlightenment. He first appraises the data from approximately 350 court cases on infanticide drawn from the years 1700–1810 in the duchies of Schleswig and Holstein. He then analyzes a dynamic debate from the second half of the eighteenth century on the publicized question, "What are the best practical means of bringing infanticide to a halt?" (p. 216). In the third part of his book, Ulbricht explores the "impact of Enlightenment ideas on the practice of criminal law" (p. 329).

Ulbricht's style can be compared to pointillism: it is rich with details, and one must step back to gain a clear impression of the large patterns. It is not always possible to bring the themes into sharp focus. This is especially true in part 1, dealing with the social history of infanticide. Ulbricht defines his research arena as Schleswig and Holstein but intersperses material from all of Europe. Presumably the author intends this as a means of supplementing the picture he could construct from local records, but at times the weaving of information from disparate sources is more confusing than clarifying.

The second section of the book, dealing with the Enlightenment debate, is more sharply delineated and will add to a scholarly understanding of late-eighteenth-century ideals. Since ideas are not confined by political boundaries, it is appropriate that Ulbricht draws material from a wide context. In the book's third section the author mingles details from the two northern duchies with wide-ranging evidence from elsewhere. The data is rich, but it is diffusely presented.

Dülmen paints his account with bold strokes, offering generalizations about three centuries of historical experience. Each of his chapters represents a topic: social and cultural ideas regarding infanticide; judicial investigation techniques; capital punishment; frequency of the crime; and the Enlightenment discourse. The author employs archival sources, including data from the cities of Nuremberg, Danzig, and Würzburg. He also draws on his own substantive published work in early modern social history, which addresses themes such as poverty, love, crime, and household life. Specialists may be unconvinced by some of the sweeping conclusions reached in such a small volume, yet a strength of the account is its wide-ranging narrative.

Dülmen points out that many contemporaries were convinced that infanticide was increasing with alarming frequency. As the author indicates, however, this perception can never be checked against archival evidence, which records only instances reported to authorities and bears no relationship to the actual number of cases. What is clear is that the number of actually documented infanticides multiplied dramatically after the mid-sixteenth century. Dülmen attributes this to civilian governments' increasing involvement in people's lives.

Although the books have different objectives, they agree about strikingly uniform patterns among offenders and punishments. The accused were, with few exceptions, young, working-class women. Having become pregnant through seduction, consent, or rape, they typically found themselves without male partners and attempted to disguise their condition. Immediately after giving birth they experienced desperation, fearing that discovery would mean loss of both honor and livelihood. Irrationally hoping to conceal a situation that could destroy their lives, they killed their infants and ineptly hid the evidence. When discovered, they were executed for a crime considered heinous in canon and civil law. Until the late eighteenth century, courts were interested only in discovering and punishing the deed; they never took into consideration circumstances leading to the offense. This changed after the mid-eighteenth century, when jurors sometimes handed down milder sentences such as imprisonment. Before 1800 some princes experimented with the prevention of infanticide; their methods included the establishment of foundling hospitals.

The judicial response to infanticide changed dramatically over time. In the Middle Ages authorities buried offenders alive, sometimes impaling the body with a stake. Sixteenth-century law prescribed drowning the guilty; some provincial codes required that

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the convicted be drowned in sacks containing live animals. In the seventeenth century executioners used the sword, and also on occasion the wheel. Dismemberment was sometimes part of the ritual of execution. Banishment and imprisonment became common after the mid-eighteenth century.

Both Ulbricht and Dülmen present evidence that social attitudes and legal practices regarding infanticide became less cruel during the Enlightenment. The use of torture and capital punishment declined, while authorities became interested in preventing the killing of infants rather than simply punishing the mothers.

This is significant in view of new, but widely held, interpretations of gender in the Enlightenment. Evidence is conclusive that, rather than creating a less-patriarchal world, the Age of Reason invented new forms of misogyny, through the establishment of new norms of domesticity and the exclusion of women from the productive economy, for example. Ulbricht's and Dülmen's contribution to a systematic understanding of infanticide calls for a refinement in interpretations of the Enlightenment. The evidence does not justify the conclusion, however, that the Enlightenment represented an emancipation for women. In spite of humane ideals, European women continued to suffer devastating consequences when they experienced unplanned births, especially outside of wedlock. The nineteenth century contains abundant data confirming this.

The momentous question raised by the two new books on infanticide deals not with the Enlightenment, but with the early modern passion to identify and punish women accused of infanticide. In the sixteenth century, males of the ruling classes suddenly sought new ways to control women's lives. They held women of low social status responsible for what would be regarded in the twentieth century as a social problem. The craze over infanticide lasted for three centuries. Thousands of maids suffered harrowing executions, bearing the burden of a misogynous society's insecurity.

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FRANK-LOTHAR KROLL. *Friedrich Wilhelm IV. und das Staatsdenken der deutschen Romantik.* (Einzelveröffentlichungen der Historischen Kommission zu Berlin; Forschungen zur preussischen Geschichte, number 72.) Berlin: Colloquium. 1990. Pp. xiv, 238. DM 88.

Originally a Cologne dissertation prepared under the supervision of Andreas Hillgruber, this carefully crafted monograph by Frank-Lothar Kroll represents an important contribution to our understanding both of Frederick William IV and of the evolution of Prussian conservatism in the first half of the nineteenth century. It is well known, of course, that Frederick William was a "Romantic"; until now, however, virtually no historian has seriously attempted to analyze the relationship between his Romantic proclivities as crown prince and his conduct as Prussian monarch after 1840. As the title of his study suggests, Kroll deliberately emphasizes the political dimension of Frederick William's Romanticism, for it is this dimension, rather than the "aesthetic" and "subjectivist" elements of his personality, that is historically more significant. Moreover, by emphasizing the political consequences of Frederick William's Romanticism, the author casts doubt on Carl Schmitt's well-known assertion that "political Romanticism stops where political activity begins" (p. 5).

Kroll's account is divided into two parts. The first, on the "intellectual bases and preconditions" of Frederick William's political Romanticism, begins with a detailed analysis of the relationship between the Prussian state and Romanticism, which had an enduring impact on a number of young individuals who later came to occupy prominent positions in that state. Above all, the ideas of Berlin-Potsdam Romanticism indelibly influenced the impressionable crown prince, especially during the critical decade before 1823.

Berlin-Potsdam Romanticism was only one source of Frederick William's political ideas, as the author acknowledges. He also points to the important role of pietism as well as the teachings of Burke, Haller, and many others. None of these ideas, however, proved to be "of such lasting intensity" as the Romantic views that he absorbed from his teacher Ancillon or from the popular Romantic epics of Friedrich de la Motte Fouqué. The book's second chapter, in which the author carefully reconstructs the crown prince's intellectual and political coming of age after 1812–13, is by far its best. Kroll has thoroughly combed the available archives and perused virtually every word that Frederick William wrote at that time. He has also been able to reconstruct Frederick William's reading during those years.

Central to Frederick William's Romantic beliefs was an attachment to an organic, *ständisch*-corporative conception of monarchy in which the king would maintain "historic" rights against the usurpations both of revolutionary zealots "from below" and "despotic" bureaucratic centralizers and modernizers "from above." In the three chapters that compose the book's second part, Kroll attempts to show how Frederick William applied his Romantic notions to the problems of Prussian constitutionalism, the German question, and Prussia's obligations as a European power. Not surprisingly, he finds in the king's behavior an ideological consistency that belies the standard image of the monarch as an indecisive, mercurial waverer.

Thorough, concise, and elegantly written, Kroll's study is packed with arresting insights. To be sure, the first part is more vigorous and persuasive than the second, while the chapter on Frederick William's European policies seems almost to have been "tacked on" to the rest of the manuscript. Kroll also pays too

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little attention to Frederick William's artistic interests, which are inseparable from his political views. Still, these are comparatively minor quibbles and do not distract seriously from a graceful monograph that should be consulted by anyone interested in nineteenth-century German conservatism.

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ARDEN BUCHOLZ. *Moltke, Schlieffen, and Prussian War Planning*. New York: Berg; distributed by St. Martin's. 1991. Pp. xi, 352. $59.95.

In 1985 Arden Bucholz published his superb work, *Hans Delbrück and the German Military Establishment*. The present volume serves as its companion. The research base is impressive and centers on the documentary holdings of the Federal Military Archive at Freiburg and the Bavarian War Archive at Munich in Germany, as well as on the papers of generals Wilhelm Groener and Alfred von Schlieffen on microfilm at the National Archives in Washington.

The book traces the origins and growth of the Prussian war planning process from the 1790s to 1914. Its core consists of the sections dealing with Schlieffen as the re-creator of Prussian war planning (chaps. 3 and 4) and Helmuth von Moltke the Younger's execution of his predecessor's famous recipe for victory (chaps. 5 and 6). The author's ability to place Schlieffen especially within the context of his social, religious, and political upbringing should serve as a model for all historians of the period. In essence, Bucholz has provided English-language readers with a highly detailed manual of the Prussian general staff's "way of war." The author is especially adept in detailing how railroads and telegraphs altered military planning and how staff rides as well as war games were used to simulate anticipated battlefield conditions. Prussian military education, ranging from the august War Academy in Berlin to the seven cadet schools scattered throughout Germany, is aptly described, as is its shift to almost exclusive emphasis on the so-called "science of war" to the detriment of history and politics. The seven escalatory phases of the mobilization for war are laid out in masterful analytical fashion.

I have only two quibbles. Bucholz relies almost exclusively on Schlieffen's hagiographers after World War I to lay the blame for the lost campaign of September 1914 at the Marne River solely on the younger Moltke. A sympathetic reading and use of Jehuda Wallach's *The Dogma of the Battle of Annihilation* (1986) would have produced a more balanced account of the daunting task facing Schlieffen's successor after 1906. And, while the book on the whole is well written, Bucholz's attempt to couch his findings in the language of modern management manuals has resulted in sentences such as: "The reactionary modernists were nationalists who turned the romantic anticapitalism of German radical conservatism away from backward-looking pastoralism" (p. 317). Let us leave such terminology to NATO field manuals and sociologists.

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ECKHARD TROX. *Militärischer Konservativismus: Kriegervereine und "Militärpartei" in Preussen zwischen 1815 und 1848/49*. (Studien zur modernen Geschichte, number 42.) Stuttgart: Franz Steiner. 1990. Pp. 347. DM 75.

"A state that can no longer be defended should be sacrificed in order to save the army," declared the famous military theorist Carl von Clausewitz in 1808, referring to Prussia's struggle against Napoleonic France. That army's indisputable position in Prussian society was challenged after the debacle of Jena, only to be reaffirmed in the wake of the Wars of Liberation, 1813–15. But were the foundations of this army, underpinned by an elite officer corps and shaped by authoritarian and monarchical values, as firmly entrenched as they once had been? To what extent and by what means did professional military men or civilian military enthusiasts attempt to ensure the predominance of the army in a Prussian state under siege from "liberal ideas" and to promote the inculcation of the Prussian military spirit among the civilian populace in the pre-March era, or *Vormärz*? These questions lie at the basis of Eckhard Trox's interesting discussion of military conservatism.

Taking a revisionist tack, Trox argues that historians have misjudged the contribution of the army and its advocates in the early nineteenth century to the eventual formation of Prussia's Conservative Party. He dismisses the idea that the army abstained from partisan involvement and the implication that the Conservatives must therefore have been exclusively linked to Prussian Junkers and bureaucrats. Rather, Trox suggests, the development of conservatism as a political entity and cultural force in Prussia owed a great deal to the efforts of several military journalists and a variety of military associations (among them numerous veterans' associations).

The author plods through thickets of detail in discussing the emergence of a wave of military periodicals and associations, as well as the reactions of officers toward the presumed "liberalization" of the Prussian army. When he regains his momentum in the middle chapters, we are treated to an intriguing discourse on Louis Schneider, a former Berlin actor and the "spiritus rector" of two of the most influential and reactionary military publications of the 1830s and 1840s, the *Soldatenfreund* and the *Deutsche Wehrzeitung*. It is a pity that Trox does not explain how a thespian like Schneider came to hold such sway among high-ranking aristocrats, including the prince of Prussia, one of several missed opportunities in the

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book. Trox's sections on the social and geographic dimensions of associational life are quite impressive, though, as are those that examine the ways in which public debate was influenced by military journals, some covertly sponsored or sanctioned by the army.

While there is certainly much to ponder here, the book ultimately falls short of its intended mark. Among its shortcomings are its chronological inconsistencies; Trox has so much material at his disposal that dates and events are difficult to follow and as a result the crucial links between the military and civilians and between associations and the press, links all relevant to the emergence of political conservatism, remain elusive. Regrettably there are several puzzling bibliographic omissions, such as standard works by Peter Paret, Robert M. Berdahl, Hanna Schissler, and James Sheehan, the consideration of which would have lent this book greater analytical bite. These flaws are all the more unfortunate since the author has assiduously combed through no fewer than twelve archives in Poland and the former two Germanies and, to his credit, has brought much hitherto ignored material (especially in the form of military journals and newspapers) to bear on his subject. For readers with the stamina to persevere to the end, Trox's work offers a useful perspective from which to view a still underresearched and underappreciated area of German history.

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Clemens Zimmermann. *Von der Wohnungsfrage zur Wohnungspolitik: Die Reformbewegung in Deutschland 1845–1914*. (Kritische Studien zur Geschichtswissenschaft, number 90.) Göttingen: Vandenhoeck und Ruprecht. 1991. Pp. 313. DM 48.

Clemens Zimmermann offers us an authoritative view of the ambiguous record of Germany's nineteenth-century housing reformers. While the author accepts that the reformers' achievements were severely limited by political and constitutional roadblocks, he sees great significance in their efforts nonetheless. His conclusions, tentative though they are, about the abatement of the urban housing crisis in the two decades prior to 1914, place him among the West German revisionist scholars, such as Clemens Wischermann (*Wohnen in Hamburg vor dem Ersten Weltkrieg* [1983]), who paint a picture of German housing that is less bleak than the one offered by the nineteenth-century reformers themselves and by such influential recent scholars as Lutz Niethammer ("Wie wohnten Arbeiter im Kaiserreich," *Archiv für Sozialgeschichte* [1976]). The book is based on an impressive array of published sources, which the author has supplemented with selected archival materials from Potsdam, Merseburg, and Karlsruhe. Its readers will be brought up to date with West German and Anglo-American scholarship of the past two decades on the history of housing, social reform, public health, and urban planning in imperial Germany.

Zimmermann's book is divided into three sections that treat three successive phases in the history of housing reform. The first section, on bourgeois housing reform, describes the efforts of conservatives and liberals from the 1840s to the early 1870s. It recounts the publications that first defined Germany's "housing question" (p. 29) and the initial experiments in self-help and paternalism that set patterns for many decades to come. The second section introduces the social hygienists who in the 1870s and 1880s first provided compelling arguments in favor of governmental intervention in housing construction and rental markets. These public-minded physicians attempted to apply advances in bacteriology and other fields of medicine to define "illnesses of housing" (p. 97) that would explain high mortality rates among urban populations. The third section describes the efforts of a variety of scholars, public officials, civil servants, and politicians to advance a comprehensive program of housing politics from the 1890s to the eve of World War I. Zimmermann describes the Saxon General Construction Law of 1900 as the limit of what could be achieved by housing reformers in Germany prior to 1918, given the voting laws and political constellations in the German states and the states' jealous defense of their prerogatives against encroachments by the Reich. Zimmermann turns to archival materials to document the resistance to proposed national or state (that is, Prussian) housing legislation. Elsewhere Zimmermann relies on published sources, including the nineteenth-century professional journals for architecture, public health, the housing cooperative movement, and general social reform.

Many elements of this story will be familiar to readers of Brian Ladd's book, *Urban Planning and Civic Order in Germany, 1860–1914* (1990), and of Nicholas Bullock's section on Germany in the book that he co-authored with James Read, *The Movement for Housing Reform in Germany and France, 1840–1914* (1985). Zimmermann has more to say than Ladd or Bullock about developments outside of Prussia and Berlin, however, and more to say about state and national-level politics than Ladd, whose principal interest was municipal politics. For photographs and other illustrations of the housing that the reformers wanted to build or to replace, one should still turn to the work of Bullock, an architectural historian, or to the handsome book edited by Wischermann and Hans-Jürgen Teuteberg, *Wohnalltag in Deutschland, 1850–1914* (1985).

Michael Honhart
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Klaus Hess. *Junker und bürgerliche Grossgrundbesitzer im Kaiserreich: Landwirtschaftlicher Grossbetrieb, Gross-*

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grundbesitz und Familienfideikommiss in Preussen (1867/71–1914). (Historische Forschungen, number 16.) Stuttgart: Franz Steiner. 1990. Pp. 354. DM 78.

In 1931 Kurt Tucholsky wrote sarcastically that although German agriculture had been living on the edge of extinction for the past quarter century, it now seemed to be doing "quite well" (p. 316). Now Klaus Hess has written an important study suggesting that Tucholsky came closer to the mark than he knew.

In recent years the history of the noble landowning class in Germany has come into sharp focus thanks to such books as Francis Carsten's *The History of the Prussian Junkers* (1989) and Robert Berdahl's *The Politics of the Prussian Nobility* (1988). Other studies have addressed the theme of peasants and lords in German history and the changing contours of German conservatism. These works have provided much of the social and political context necessary to understand the Junkers' fight for economic survival. Conversely, they have also demonstrated that the political *Weltanschauung* of Prussian nobles was rooted in the day-to-day management of their estates.

In Hess's view—although not in this reviewer's—much of the scholarship on the Junkers has hinged on two questions: whether a "permanent crisis" confronted German agriculture after the early 1870s, and whether that crisis was responsible for the preservation of a social class that was no longer economically viable but managed to remain at the top of Germany's social and political hierarchies well into the twentieth century, thereby helping pave the way for National Socialism (p. 16). This view was advanced by Alexander Gerschenkron and Hans Rosenberg in the 1940s and informed much of the critical social history commonly associated with the University of Bielefeld (for example, Hans-Jürgen Puhle's *Von der Agrarkrise zum Präfaschismus* [1972]).

Unfortunately, Hess forgoes the opportunity to address the broader questions about property, class, and ideology that were first raised in these provocative books and that continue to animate the best research in the field. In his brief introduction he states unfairly that previous work on German agriculture has tended to be of two sorts: on the one hand, a large number of "handbooks" and narrow monographs providing superficial or very detailed accounts; on the other, a few works with a "broader perspective that seek to clarify the reciprocal relationships between economic development, rural society, and agrarian politics" (p. 17). Among the latter, the author cites pioneering studies by Rosenberg, Puhle, Jens Flemming, and Hanna Schissler, and he seems to prefer this second genre. Yet, relative to these works, Hess's study is disappointing, for it tells us little about the survival skills of the Junkers themselves.

What Hess does, he does well. He examines the legal status of large estates (*Rittergüter* and *Fideikommisse*) together with their debt structures, tax exposures, profit margins, marketability, and geographical distribution (mainly in the six eastern provinces of Prussia). This analysis is based principally on statistical sources and other official publications, and is bolstered with 114 tables and charts. Hess concludes that the Junkers conjured up the notion of an agrarian crisis where none existed. German agriculture, he argues, was not doing so badly that large landowners in either the western or eastern provinces of Prussia were forced to sell their estates en masse. Nor were agricultural prices so low or debts so high as to substantiate the politically opportune myth of an agrarian crisis. This holds true even for the 1890s, Hess tells us, when the Junkers first mobilized the rural sector for their own political defense: "to put it simply, agriculture in the *Kaiserreich* experienced an efflorescence that may have undergone market fluctuations but which was in no way interrupted by a serious crisis" (p. 315). In any case, he adds, to speak of large landowners as a unified group in economic or social terms is misleading because the patterns of agricultural landholding and production varied enormously across the Reich.

This conclusion is significant and merits attention. It substantiates the view that the Great Depression of 1873–96 was largely mythical, and it highlights important regional disparities in German social and economic development. Sadly, however, this study may become another handbook whose main argument need not be properly digested to be cited. By advertising the summaries at the end of each chapter—intended to help the "reader in a hurry" assimilate his main points—the author implicitly sanctions such use of his book. Perhaps he knows best.

James Retallack
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Katherine Roper. *German Encounters with Modernity: Novels of Imperial Berlin.* (Studies in German Histories.) Atlantic Highlands, N.J.: Humanities Press International. 1991. Pp. ix, 269. $45.00.

Germany's second empire confronted the novelists in Berlin with the twin phenomena of nationhood and modernity. Questions both of national identity and the meaning of modern life became important themes of their fiction. Of particular note was their use of Berlin as a setting, a focus of the contending forces of German society; city landmarks were treated as symbols of hope or despair in the face of social change.

Katherine Roper analyzes about fifty titles, "consisting primarily of middlebrow novels," and maintains that they "represent significant instances of social discourse" (p. 5). For example, in Julius Rodenberg's *Die Grandidiers* (1878), a Berlin family descended from Huguenots is shown in turmoil over the Franco-Prussian War. The father cannot deny his lifelong sympathies for the French. In the end, however, he is reconciled to Prussia. Having him lay a wreath before

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a statue of the Great Elector, Rodenberg portrays an individual celebration of victory and symbolically promotes a German state with "Prussian tradition in its center" (p. 47).

Roper documents a major shift in tone as the empire wore on. Filled with the confidence of 1871, the earlier novelists tended to present unification as the portent of "a truly German revolution . . . a spiritual or moral transformation in the nation" (p. 3). But by 1900 there were "signs of confusion and fatalism," as the novels came to terms with "cultural legacies, historical mythologies, class prejudices, and political indifference" (p. 3).

Historians are sure to note that Roper has provided yet further evidence that the culture of the empire was not full of nostalgia for preindustrial, traditional life. The Berlin novelists applauded modernity, or they acquiesced in it; where they abhorred it—in "rage at a society that smothers the individual with its conventions" (p. 138)—there was no endorsement of a return to the past. Moreover, specialists concerned with the development of attitudes toward Jews, women, the working class, or city life will be fascinated by Roper's chapters on these subjects.

The opening discussion of theory and method (especially pp. 4–7) seems rather forced and remote from the body of the work. Roper insists on the relevance of "authorial intent," yet proceeds to offer many of her analyses without referring to it. Furthermore, the theoretical emphasis falls so heavily on "episodes" that one awaits the interpretation of individual scenes with no connection to the larger narratives in which they appear. That the book goes on to neglect this premise is to its credit.

Some will doubt Roper's judgment in largely ignoring the issue of reception. Some may miss—even in a monograph—a fuller discussion of the implications for understanding imperial culture as a whole. Still, Roper's book performs its central task quite well: it presents careful and convincing interpretations of the novels in question. Taking a basically conventional approach to the relationship of history and literature, it locates passages that comment emphatically on surrounding political, social, or cultural trends, and thus establishes that its material was "part of the identity formation of Imperial Germany" (p. 4).

R. E. Sackett
University of Colorado, Colorado Springs

Derek S. Linton. *"Who Has the Youth, Has the Future": The Campaign to Save Young Workers in Imperial Germany*. New York: Cambridge University Press. 1991. Pp. xi, 319. $49.50.

Previous studies of the youth movement in Imperial Germany have concentrated on the middle-class *Wandervogel*. The classic works are those by Walter Laqueur, Peter Stachura, and Hermann Giesecke. Derek S. Linton departs from this pattern by engaging in a fruitful examination of bourgeois efforts to save young workers from the dangers of industrialization and modern urban life. His three-part analysis covers the growing perception among middle-class "youth savers" in the 1890s that young workers represented a social problem; the formulation of institutional solutions by municipal politicians, urban professionals, and clergymen around 1900; and, finally, the actual implementation of reforms in the last decades of the empire. This well-written, solidly researched study focuses on the experience of the industrial continuation schools, the Protestant, Catholic, and Socialist youth clubs, and the militarization of "youth salvation" efforts that occurred after the state's Youth Cultivation Decree and the founding of the Young Germany League in 1911.

Linton develops his study along the historiographical lines of Richard Evans, Geoff Eley, and David Blackbourn. He interprets the early phase of the youth salvation movement as a middle-class attempt to maintain hegemony in a rapidly changing society. Institutions such as the continuation schools and religious clubs did not emanate from the ministries, but rather were typically liberal solutions to local problems. Although Linton views the youth savers' denunciations "of youthful wildness, delinquency, and moral degeneration" (p. 226) as exaggerated expressions of their own anxieties about the future and fear of losing control, he concludes that the vocational schools and church clubs recruited significant numbers of young toilers and essentially managed to integrate the urban youth into society—thus supporting Vernon Lidtke over Guenther Roth. Linton's monograph is especially strong in its analysis of the late and lackluster Social Democratic attempt to organize its own young people. There are interesting glimpses here of largely different party, union, and workers' cultures—differences that allowed sons and daughters to respond more enthusiastically than their parents, for instance, to the nationalistic drumbeating of the immediate prewar era.

In making this latter point, the author carries his study over a hotly contested historiographical border. To his credit, Linton realizes that the patriotism of German youth was the complex result of "a controversial and cobbled-together conjunctural reaction by state officials and local youth savers" (p. 232), particularly after 1911. That "bottom-up" and "top-down" forces coexisted in Imperial Germany, however, is admitted very grudgingly. What I miss most in Linton's study, in fact, is a more systematic attempt to bridge the feuding English and German schools. If historians of Germany have reached a historiographical "impasse," as Thomas Childers has argued ("The Social Language of Politics in Germany," *AHR* 95 [1990], 331–58), this is not entirely the result of mutually incompatible theses but rather the product of an unwillingness to cross those bridges that are

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open. Linton hesitatingly joins the few historians who are beginning to venture across.

ERIC DORN BROSE
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URSULA HERRMANN *et al. August Bebel: Eine Biographie.* Berlin: J. H. W. Dietz. 1989. Pp. 763. DM 24.80.

Written by an "author's collective" of more than a dozen historians from the former German Democratic Republic (DDR), this well-researched but ideologically tendentious book will remain the definitive Marxist-Leninist interpretation of August Bebel. It is more detailed than the recent biographies by William H. Maehl (1980) and Brigitte Seebacher-Brandt (1988), and it differs sharply from them on almost every matter of interpretation. Unlike these two earlier investigators, Ursula Herrmann and her associates present Bebel's career as proof of the ideological triumph of Marxism in the German workers' movement.

Herrmann's study excels in depicting those aspects of Bebel's career that conform to this thesis of the triumph of Marxism. Bebel's early experience with poverty, his work as a manual laborer, his avid, life-long interest in socialist theory, his long and fruitful collaboration with Friedrich Engels, his persistent expectation of the imminent collapse of the capitalist order, his indictment of capitalism as the principal cause for the exploitation of women and for the perils of militarism, and his fight against reformism in the major intraparty battles between 1885 and 1905 are all mustered here to demonstrate Bebel's contribution to the victory of Marxism.

This book is less illuminating on those aspects of Bebel's career that do not conform to its theme. Organizational ties to the left-liberals in the 1860s, the persistent influence of Ferdinand Lassalle in the 1870s and even later, the spineless reaction of the party to the imposition of the antisocialist law, and evidence of reformism in Bebel's own speeches are all glossed over. In some cases, the authors obscure Bebel's heterodoxy with dubious interpretations. They unconvincingly construe Bebel's repeated public pledges of working-class support for a war against Russia, for example, as having been inspired by the expectation that a war-provoked crisis could generate a proletarian seizure of power.

In this and in countless other cases, Herrmann and her collaborators quote Marx, Engels, or Lenin to establish the propriety or the error of Bebel's thoughts and deeds. Today, with so many formerly communist parties repudiating Marxism-Leninism, this practice seems downright archaic. In this book, it produces the bizarre result that Lenin is mentioned more than Lassalle.

Also in the style of earlier, polemical East German writings, these authors refuse to reply to arguments published by Western historians. Instead they rely almost exclusively on those studies that share their monolithic interpretation of the German workers' movement. One hopes that, with the demise of the DDR and the recent transmogrification of its rigidly orthodox Institute for Marxism-Leninism (which formerly housed this author's collective) into the Institute for the History of the Workers' Movement, open intellectual discourse will replace this blindered scholarship.

These criticisms notwithstanding, this biography is more balanced than some earlier Marxist-Leninist work. The authors give a nuanced treatment to Bebel's tactical explorations of the 1890s, which included the apparently contradictory elements of a harsh denunciation of Georg von Vollmar's reformism alongside a determined endorsement of an agrarian program that promised relief to small farmers. The authors also provide considerable information about Bebel's private affairs, including an interesting evaluation of his growing personal fortune. The value of this book is further enhanced by ninety-six pages of excellent illustrations. But since its ponderous length makes it more likely to be used as a reference book than as popular reading, it is regrettable that it lacks both a bibliography and a subject index.

RAYMOND H. DOMINICK
Ohio State University, Mansfield

BERNARD DEGEN. *Abschied vom Klassenkampf: Die partielle Integration der schweizerischen Gewerkschaftsbewegung zwischen Landesstreik und Weltwirtschaftskrise (1918–1929).* (Basler Beiträge zur Geschichtswissenschaft, number 161.) Basel: Helbing und Lichtenhahn. 1991. Pp. 333. DM 68.

Bernard Degen's monograph on trade unions, wages, and working hours in Switzerland is a contribution to the history of "the most agitated years in the history of the Swiss workers' movement" (p. 10), from the end of World War I to the onset of the Depression. Using published sources, he argues that the general strike of 1918, which brought the forty-eight-hour week to Swiss industry, decisively integrated labor into Swiss society and "defused social conflict and the danger of new mass actions" (p. 11). According to Degen, what happened at the negotiating tables at which representatives of industry and labor sat in the immediate postwar years was no less than the Swiss trade unions' "departure from the class struggle" (p. 110). He locates the political and economic concessions workers won within the context of European-wide instability after the war, which necessitated a "new integration" (p. 289) of labor in the era of postwar mass politics.

Yet labor's awakening was shortlived. The severe economic slump of 1920 swiftly altered the balance of power between capital and labor in Swiss society in

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favor of the employers. Employers mounted a counteroffensive, targeting wages and hours. They wanted to restore the competitiveness of Swiss industry, whose problems, Degen notes, were related to the world recession and not to any local failure. A trickle of exceptions permitting the lengthening of the work week, allowed under the Swiss factory law of 1920, became a torrent, proceeding apace with the demoralization of Swiss labor in the face of reinvigorated bourgeois hegemony. As both worker resistance and collective bargaining agreements melted away, employers pushed ahead with wage reductions, although Degen provides evidence that the decline in real wages may not have been so precipitous.

In the political arena, the Left's decline was initially less dramatic; 57.6 percent of the voters in a popular referendum in February 1924, for example, refused to overturn the legal underpinnings of the forty-eight-hour week. Yet growing demoralization sapped the efficacy of the trade unions throughout the 1920s. By the onset of the Depression in 1929, many Swiss firms had achieved a fifty-two-hour week and reduced hourly wages. According to Degen's account, with his argument on behalf of Swiss exceptionalism, the achievements of Swiss labor in the postwar decade seem puny indeed when compared to those of British, German, or Scandinavian trade unions. In Switzerland, labor's "full acceptance" (p. 305) as a negotiating partner had to await the end of World War II.

As labor history, this is not a very sophisticated study, although Degen shows on occasion that he understands the broader texture of Swiss society and European development in the 1920s. There is virtually no discussion of changes in production processes or of other issues related to industrial rationalization in the 1920s, either in a national or a comparative international perspective. The regional dimension within Switzerland itself is underdeveloped, although not wholly lacking. Degen's handling of trade union politics shifts somewhat haphazardly between factory base and both local and central organizations. Finally, despite the subtitle, there are only a few lines devoted to the years after 1925. Nevertheless, with its solid empirical foundation, this book will be welcomed by historians of European labor during the era immediately after World War I.

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Georgia Institute of Technology

Jill Lewis. *Fascism and the Working Class in Austria, 1918–1934: The Failure of Labour in the First Republic.* New York: Berg; distributed by St. Martin's. 1991. Pp. x, 236. $58.50.

Few historians of the Austrian labor movement in the First Republic would dispute Jill Lewis's contention that a singular focus on Viennese labor cannot "offer a full explanation of the fate of the national movement as a whole" (p. 10). This brief monograph attempts to redress the imbalance by paying greater attention to Styrian developments. The result, however, is neither a detailed local study nor a significant reinterpretation of the First Republic's history, but a mixture that fully serves neither purpose. Although Lewis frequently compares the situation in Styria to developments in the capital, there are few comparisons to other provinces. This omission is especially unfortunate in the case of Upper Austria, where there was a parallel radicalization of the party Left and the Schutzbund in the early 1930s (leading to the outbreak of the civil war in Linz). Thus, it is never entirely clear to what extent Styria provides a unique example of the party's failure to respond to the situation in the province, or whether the case illustrates the general weaknesses of Social Democracy on the defensive.

Any study of the Styrian labor movement must take into account the importance of the Alpine Montangesellschaft, Austria's largest iron and steel conglomerate, Styria's major employer, and a key political actor. Despite some interesting vignettes, the role of the firm and its policies toward workers, especially the "fascist" Independent Unions, occupies only a small part of Lewis's narrative. Nor is there sufficient attention to Lewis's most important theme, the growing "spectacle of workers in Heimwehr uniform" (p. 8). Much of the book is taken up with familiar aspects of the Viennese party and its political fate, adding little apart from the conspicuous contrast with Styria where there were few social welfare projects, and where after 1928 the weak socialist free trade unions were largely displaced by the Independent Union, dominated first by the Heimwehr and later by National Socialists.

Lewis's most original chapters focus on the aftermath of the bungled "Pfrimer Putsch" of September 1931. She rightly places a good deal of blame for the rapid nazification of the Styrian Right on the Alpine Montangesellschaft, which came under German control in the early 1920s. Especially after 1933, when management and staff switched their allegiances from the Heimwehr to the Nazis, the company politicized its employee policy, giving preference to Nazi workers and provoking a hostile response even from pro-Heimwehr workers who openly displayed their "frustration and anger" (p. 189). Lewis points out that at the time of the militant mineworker strikes of September 1933, Heimwehr membership was as high as 90 percent and many workers were openly sympathetic to the banned Nazi Party. This fact attests to the volatile political trajectory of many Styrian workers from the Social Democrat Party to the Heimwehr, and, ultimately to National Socialism in the course of a few short years. Thus, Lewis's claim that "Styria was not a province in which the mass of working people became ideologically committed to fascism" (p. 207) hardly does justice to her own evidence. Fascist success among Styrian workers still remains one of the

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murkiest areas in the historiography of Austrian socialism.

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CONAN FISCHER. *The German Communists and the Rise of Nazism*. New York: St. Martin's. 1991. Pp. xiv, 285. $45.00.

It is now a commonplace to observe that political extremists of left and right have much in common. Conan Fischer maintains that right-wing radical organizations, including the Nazi Party (NSDAP), made substantial inroads into the German working class during the Weimar Republic, and he seeks to determine how the Communists responded to the threat of competition from the Right. In part, his answer is that Communists sometimes sought to play down their differences.

Fischer first provides a not entirely smooth overview of the history and political maneuvers of the leadership of the Communist Party of Germany (KPD) from the founding of the Weimar Republic until 1928. The evolving relationship between the KPD and Moscow, factional disputes within the KPD Central Committee, and Communist reactions to a range of rather different rightist organizations all interfered with an accurate assessment of the Nazi movement. From 1928 on, the Nazis became much more prominent, and Fischer has more and better evidence, often from police records, of Communist reactions to Nazism among grassroots members and local functionaries.

Moscow's directives to the KPD Central Committee after 1928 to concentrate its attacks on "social fascists," that is, the German Social Democrats, clearly diverted German Communist attention from the Nazi threat and made effective Communist (not to mention Social Democratic) countermeasures far more difficult. At the same time, however, contacts between Communists and Nazis in the factories, the streets, and working-class neighborhoods made it impossible for the KPD to overlook two facts: that the two extremist movements were often in direct competition for members and voters; and that a more flexible ideological and propaganda line might win some pro-Nazi workers, as well as the lower-middle class, over to the Communist side.

Some Communist workers and functionaries considered their Nazi counterparts less conservative and more open to argument than Social Democrats or the socialist trade unionists. Parties of protest appealed to those of a certain temperament and generation, whatever their class background, and Fischer notes a slight broadening of the KPD's social base during the early 1930s. The Communist belief that working-class Nazis had misunderstood their true class interests and/or fallen victim to deceitful Nazi propaganda only increased the temptation among Communist officials to make a major effort to proselytize among the NSDAP rank and file. Even the increasing number of violent clashes between Storm Troopers and Communist militants did not always create permanent or unbridgeable antagonisms.

Fischer highlights Communist willingness to exploit nationalist sentiments, which, however, proved to be a dangerous tactic. Attempts to encourage fraternization among Communists and Nazis on the basis of common nationalist and anti-system sentiments could and sometimes did make unsophisticated Communists more susceptible to Nazi appeals. As Fischer recognizes, the Nazis were organizationally better positioned than the Communists to succeed in a competition of look-alikes.

Although Fischer draws on recent findings about the broad social composition of the Nazi movement, he is curiously reluctant to set his findings about Communist perceptions of and reactions to Nazism in historiographical context. There are also some glaring omissions from his bibliography (for example, works by Hermann Weber). All the same, the historian of German Communism needs to go beyond study of the political and ideological pronouncements from Moscow and Berlin, and this book adds to our understanding of Communist difficulties in the late Weimar period.

RICHARD BREITMAN
American University

MARLIS STEINERT. *Hitler*. Paris: Fayard. 1991. Pp. 710. 180 fr.

Marlis Steinert has produced yet another biography of Hitler. This book is included in a series of historical biographies issued by the French publisher Fayard, featuring such prominent past figures as Richelieu and Metternich. Steinert, perhaps best known in the English-speaking world because of her *Hitlers Krieg und die Deutschen* (1970; published in the United States in 1977 as *Hitler's War and the Germans*), admits in her introduction that anyone who offers a new biography of Hitler after "Konrad Heiden, Alan Bullock, Joachim Fest, Werner Maser, and John Toland, to name only a few of the best known" (p. 7), must justify a project mapped out on such well-surveyed ground.

Steinert claims to provide a balanced view of Hitler somewhere between the earlier "demoniacal" views of the man and later nearly "mythical" perspectives (p. 8). She also believes that, within a field where there is a massive outpouring of literature, it is necessary periodically to issue reevaluations, including the fruit of the labors of more recent authors as well as the new perspectives of social history. She includes relatively recent comparisons and contrasts between the uniqueness of the German historical

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path (the *Sonderweg* or alternate path to modernity) with the "socio-structural" destinies worked out by other European societies (p. 9). She sets out, therefore, to paint a general biography in broad strokes, supported by the best of recent writing as well as some archival research. Her book accomplishes her goals.

As with most recent works on Hitler, Steinert spends considerable time on the formation of his quite Germanic yet simultaneously atypical personality. Here, rather artfully, Steinert merges personal factors with the broader European matrix, thus keeping her promise to provide a sociostructural context for Hitler himself as well as for the decline of Weimar. The methodology draws on early works like that of Heiden (*Der Fuehrer* [1944]), the later psychohistorical effort of Robert G. L. Waite (*The Psychopathic God: Adolf Hitler* [1977]), and the psychological approaches of authors like Eric Fromm. These are merged with testimony from the memoirs of those in the movement who knew Hitler well and the Führer's own *Mein Kampf*. When she concentrates on the development of the Nazi Party and the demise of Weimar, she thoroughly lives up to her announced intent of including the most recent works on the subject (many published during the 1980s). As she proceeds to the discussion of Hitler as chief of state and chief of war, Steinert relies a bit more heavily on documents, skillfully analyzing sources even as she continues to generalize based on the most recent scholarship.

The portions on Hitler himself (part 1) and his work as chief of state (part 2) are the strongest sections of the book. Problems of totalitarian manipulation through propaganda and coercion (as I observed some years ago when reviewing *Hitler's War and the Germans* for another journal) have long been Steinert's strength, and her abilities in such areas are displayed well in part 2. Part 3, dealing with Hitler as war commander, is necessarily more traditional. It is difficult to escape the step-by-step progress into war and all the usual stages describing the ascent and descent of German arms until 1945. Lastly, in a short epilogue, Steinert highlights some of the massive contradictions in the Hitlerian style, such as his relations with women, views on the military, and his "enchantment" with artists (pp. 575–77).

This is a relatively concise biography as Hitler studies go, yet it is more penetrating than some of the larger works. The last question is: should it be translated into English to make it available to a broader audience? That question should be answered in the affirmative.

Otis C. Mitchell
University of Cincinnati

Detlef Mühlberger. *Hitler's Followers: Studies in the Sociology of the Nazi Movement*. New York: Routledge. 1991. Pp. xii, 276. $57.50.

This book presents data on the social backgrounds of rank-and-file Nazi Party (NSDAP) members, the Storm Troopers (SA), and the SS. Detlef Mühlberger, who has analyzed material from regional archives, provides an enormous amount of information on party members' occupations. His main concern is with the party's class composition: whether it was a middle-class party (*Mittelstandspartei*) or a mass movement (*Volkspartei*) drawing generally from all levels of the society.

The first chapter provides a rather loose review of these polar positions, together with a justification and description of the twenty-one occupational subgroups used in the analysis. For convenience, in much of the subsequent discussion subgroups are combined to form four categories: lower class; middle class (combining lower and middle-middle segments); a combined upper-middle and upper class; and five subgroups listed as "status unclear." The six main chapters present membership data for four regions (Western Ruhr, Württemberg, Hessen-Nassau-South, and South-Hanover-Brunswick) and for some local units of the SA and the SS.

Mühlberger draws two major conclusions from this sweeping review of regional membership lists: the "lower class" (most previous commentators refer to this segment as the working class) was strongly represented, more so than many historians have suggested; and the "middle class" was at best only marginally overrepresented, and thus was less important than some have claimed. The party, in short, was a mass movement.

These conclusions, on the whole, appear to be well founded. But it is difficult to see much of a contribution to the historiography in these meager results. The same findings were reported in Hans Gerth's early study ("The Nazi Party: Its Leadership and Composition," *American Journal of Sociology* 45 [1940], 517–41) based on national membership figures. The basic problems were signaled there also: questions about classification and the extent of over or underrepresentation. Comparable results also appeared in Michael Kater's comprehensive review (*The Nazi Party* [1983]). Mühlberger is right to object to Kater's reading of his findings as supporting the *Mittelstand* position. Kater's results, however, showed the pattern again revealed in this book. Mühlberger's basic argument, therefore, is not surprising. That we now have an array of details for Aalen, Altensteig, Backnang, Balingen, Biberach, and many other communities (this from a twelve-page table including findings for one region) is a limited return, for both researcher and reader.

More than one-quarter of the 209 pages of text are devoted to maps and statistical tables. The main tables list the percentages of members found in the twenty-one occupational categories in a large number of communities in the districts covered. Each of the data chapters, moreover, contains a table showing the percentages of members in the occupational catego-

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ries and in five sizes of community. The substantive yield of this exercise is very limited.

The bulk of the text describes results shown in the tables. The book, accordingly, proves to be a dreary, extreme case of description without analysis. Even when the author is not reviewing tables, the same basic absence of thought is indicated. One sentence may give a feeling for the work's substance and style: "The long organizational history to which the Nazi Movement could look back upon in this part of county Wesermarsch is reflected not only in the large number of SS members who could date their Nazi Party membership back to 1928, and 1926 in one instance, but also in the fact that 41 per cent of the SS members recorded in the muster rolls of Sturm 3/I/24 by the time entries ceased in November 1934, had joined the SS before 30 January 1933" (p. 194). That sentence appears in the middle of a dense descriptive paragraph stretching over three pages.

The extensive footnotes contain a commendable list of studies of the party's membership, voters, organizations (both national and regional), and activities. Unfortunately, only limited use is made of the insights and observations contained in this vast literature. In the initial review of positions, one would expect to find Thomas Childers listed as the most obvious current exponent of the *Volkspartei* position: although Childers is cited later, no reference is given to his work in the introduction. Sigmund Neumann, who devoted many pages to the threatened and susceptible *Mittelstand*, is here identified with the opposite position. Knowledgeable readers would recognize the obvious relevance of Jürgen Falter's many publications, but Falter's work is signaled only in the last pages of the text and explicitly only in a late footnote.

Clearly an immense amount of work has gone into this book. It is regrettable that the intellectual yield is so limited. The author has kept so close to his narrow purpose that he has seen little else. One of the original positions in the debate about Nazi class positions is more appropriately termed the *lower*-middle-class thesis. That allegedly marginal and threatened segment was long believed to have given heavy support to the NSDAP. But from the initial discussion and design of his research, Mühlberger has passed up the opportunity to make a contribution to this debate. Some of the membership lists he used, it seems, contain information on religion, a rare datum in such files. Although Mühlberger recognizes its importance, no findings are presented, that possibility too being bypassed. The sweeping claim in the final paragraph—"Nazism [was supported] by people from all walks of life, by Catholics and Protestants"—comes without manifest evidence. Reference is made to the party's ideology, which is said to be "contradictory and cretinous" (p. 209), but there is nothing on its role in attracting members.

Mühlberger sees a "need for further detailed analysis of the social base of the NSDAP at the regional and local level to provide a more comprehensive geographic coverage than that which is provided here" (p. 208). More of the same, he announces, will appear in subsequent publications.

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RICHARD BREITMAN. *The Architect of Genocide: Himmler and the Final Solution*. New York: Alfred A. Knopf. 1991. Pp. xii, 335. $23.00.

Richard Breitman begins this important book by reviewing the controversy over the origins of the "Final Solution of the Jewish Question": the debate between those who see the massacre of European Jewry as a carefully planned program flowing directly from Nazi, and especially Hitler's, ideology, and those for whom murder was improvised in the course of the war against the Soviet Union, after the failure of other Nazi attempts to deal with the Jews. "Scholars on both sides of this debate have made serious errors" (p. 25), Breitman declares, in the authoritative tone he adopts throughout. Closely tracking the declarations and activity of Heinrich Himmler, who he argues was the key person carrying out this mass murder, the author claims to set the record straight.

While accepting that the Final Solution was not "predetermined," Breitman nevertheless believes that the Nazis' prewar rhetoric signified an intention as early as the end of 1938 to kill Jews, on the part of the SS at least. Up to the outbreak of war, Hitler's own murderous impulses were restrained by the idea that the Jews could be used as hostages in bargaining with the West. But once fighting began, and particularly once a compromise peace appeared impossible, the Führer and his SS chief turned to European-wide mass murder. "Events and documents from early 1941 suggest that Hitler had made a fundamental decision to exterminate the Jews," Breitman writes (p. 153). By March of that year—a much earlier date than that identified by most specialists—he contends that planning began, and "the Final Solution was just a matter of time" (p. 247).

But the key interpretive problem, as the author notes himself, is that the "surviving documents may not reflect the full reality" (p. 27). An incomplete and sometimes fragmentary record, the Nazis' own secretiveness, and Hitler's peculiar style of decision making prevent absolute certainty and permit a range of plausible interpretations, although this range has been narrowing recently as a result of significant publications by Christopher Browning, Eberhard Jäckel, Philippe Burrin, and others. How convincing, one must therefore ask, is Breitman's new analysis?

Examining Himmler's private papers and those of his various agencies, the author charts the decisions and political maneuverings of this romantic, sometimes wooly minded racial fanatic who was strongly

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loyal to Hitler until the war began to go badly. Breitman shows how so much of Himmler's Jewish policy, as much else that he did, was attuned to fortifying the role and standing of his SS against its principal rivals—the SA, particularly at the time of *Kristallnacht*; General Johannes Blaskowitz and the Army during the Polish campaign and later; Hans Frank during 1940–41 over policies in Poland; Alfred Rosenberg and his Ministry for Occupied Eastern Territories; and various *Gauleiter* who governed in the East. Of Himmler's cruelty and ruthlessness there can be no doubt whatever, just as there is no question about his conception of Jews as a demonic force, deeply threatening to Germany and to the Germanic utopia to which he was committed.

But about Himmler's concrete intentions regarding Jews, real questions remain. Breitman, it seems to me, picks over the evidence like a man searching for gold, only in this case the precious metal is evidence of an intent to murder the Jews of Europe. As a prospector, he strains for any sign of glitter. Thus, when Himmler's words might be consistent with a murderous objective, Breitman writes as if they confirm the existence of an evolving plan. For example, in describing Himmler's order banning Jewish emigration from the Government General of Poland in the spring of 1940, Breitman seemingly rejects the Nazis' own explanation, that this was to speed Jewish departures from the Reich: "But if there was a secret political-ideological reason not to allow Polish Jews to emigrate, Himmler's order made perfect sense. The political-ideological motive was revealed several months later" (p. 101). Some contrary evidence is disparaged. On the SS planning in 1940 to ship the European Jews to Madagascar, the author prefers to see Himmler's commitment to the project as a tactical convenience: "He required a policy that would for the moment satisfy Hitler, Göring, and Frank, give the appearance to the outside world that Germany was not behaving 'irresponsibly,' and draw Jewish policy under the tighter control of the SS" (p. 122).

Himmler appears in Breitman's own reading to be a less-useful key to the puzzle of the Final Solution than might be suggested by the preeminent role of the SS. For as Breitman himself shows, the Reichsführer SS was not a strategic initiator, but rather a faithful executor of his Führer's decisions, particularly on a matter so important to Hitler as the Jewish Question. And while Breitman states boldly that by "early 1941 . . . Hitler had already made a fundamental decision to exterminate the Jews" (p. 153), this is a minority view among specialists, and one for which the author produces no convincing new evidence.

These reservations aside, I must add that I much admire this work, particularly for its resourceful combing of primary materials including Himmler's appointment books and office log, pretrial records of various war criminals, other documentation at the National Archives in Washington, and important collections in Germany. Breitman's detailed discussion reminds us of the gigantic scope of decision making for the Final Solution, involving political triumphs over Himmler's rivals, requiring elaborate preparation of killing procedures, and necessitating extensive mobilization of subordinates within the SS. Thereby he provides a convincing demonstration of how the Final Solution evolved gradually during the latter part of 1941—one of the original contentions of the functionalists, to which Breitman now adds his considerable authority. Whether or not one agrees with its central argument, therefore, there is much to learn from this book about the Final Solution, its origins, its implementation, and its hate-inspired architect.

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Ottavio Clavuot. *Biondos* Italia illustrata—*Summa oder Neuschöpfung: Über die Arbeitsmethoden eines Humanisten.* (Bibliothek des Deutschen Historishchen Instituts in Rom, number 69.) Tübingen: Niemeyer. 1990. Pp. viii, 406. DM 118.

Flavio Biondo's *Italia illustrata* constitutes the middle, and until now least studied, of the three great projects he undertook during his mature period as a leading humanist of Quattrocento Italy. In *Decades* (ca. 1435–43) he essentially invented the medieval history of Italy, and his *Roma triumphans* (1457–59) pioneered the analysis of ancient Roman institutions; in *Italia illustrata* (1448–53), he conceived a historical geography of the Italian peninsula, examining region by region the topography, the pattern of urban settlement, the political experiences, and the cultural accomplishments of its people. He composed the work while in temporary disgrace at the Roman Curia, which led to an itinerant existence that took him from his native Romagna to Milan, Venice, and ultimately Naples. These journeys permitted a personal reacquaintance with large parts of Renaissance Italy, retracing much of the ground he had first traversed during two decades of student and early professional life. None of Biondo's humanist contemporaries possessed such extensive personal knowledge of the terrain and sites of northern and central Italy. The *Italia illustrata* reflects this first-hand familiarity, but it is as well a major achievement of humanist scholarship. This argument forms the focus of Ottavio Clavuot's monograph, the first book-length study since the turn of the century devoted entirely to this text.

Clavuot's purpose is to examine the aims, source materials, and working methods Biondo employed in the *Italia illustrata* in order to assess whether it essentially involves a compilation of existing medieval traditions (a "summa") or whether it forms in fact a new creation of humanist scholarship (*Neuschöpfung*). In a sense, he concludes it is both. Biondo did draw on existing medieval works, including chronicles,

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maps, itineraries, and lists of place names, and on the tradition of the *descriptio urbis* for themes and subject matter. Yet his most extensively utilized sources were classical. A major strength of Clavuot's analysis is his close examination of the specific sources Biondo had at his disposal and how he used them. Detailed line-by-line comparisons and an extensive critical apparatus clearly reveal how Biondo worked. Biondo did resort to such newly developed humanist methods of critical historical inquiry as epigraphy, archaeological evidence, and lexicography. His principal sources, however, were literary texts, above all Livy's history and Pliny's *Naturalis historia*.

Yet Clavuot believes Biondo's most notable achievement was to transform these disparate ancient and medieval materials into a coherent whole, influenced in significant ways by the classical rhetorical genre of urban panegyric. Perhaps most telling, Biondo departed from the sacral mythology of medieval geography to set forth a secular topography of a land shaped over the centuries by human intentions and actions. To this end, he was particularly concerned to identify the precise historical circumstances in which cities had been founded and to trace the historical vicissitudes they had experienced. For Biondo, Italians shared a living heritage, shaping a common culture, the lineaments of which could be established by critical human inquiry. Clavuot's study establishes convincingly the intellectual merits of the *Italia illustrata* and thereby contributes to Biondo's growing reputation as a humanist of the first rank.

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FRANCESCO CARACCIOLO. *Sud, debiti e gabelle: Gravami, potere e società nel Mezzogiorno in età moderna*. Messina: Sfameni. 1989. Pp. 404. L. 45,000.

This book is the unrevised reprint of a work first published in 1983. In it, Francesco Caracciolo presents an enlarged version of materials and interpretations first published in a 1963 article. Caracciolo is one of few historians who have studied local finances and taxation in the kingdom of Naples during the Spanish period (1503–1707), and his thesis is a clear one.

The royal government, as was common in many areas of Europe, determined on the basis of censuses the fiscal obligations of each community, but then left each community to decide how to levy its taxes and distribute the fiscal burden within its population. In the sixteenth century, Caracciolo argues, most communities relied heavily on direct taxes on the income and property of their residents and on the head tax paid by all non-nobles. Between roughly the 1580s and the 1620s, the end of economic prosperity in the kingdom and the growing fiscal pressure of the Spanish government led to large indebtedness on the part of many communities and moved the local elite—who, according to Caracciolo, controlled local institutions—successfully to press for a switch to a system based primarily on indirect taxes on consumption (the *gabelle*). These affected disproportionately the poorer members of the communities. Although his book includes a brief discussion of the fiscal system of the city of Naples, and a critique of Villari's views on the failure of the royal government to protect the provincial population from the pressures of the elite, this switch from one fiscal system to another is central to Caracciolo's interests. The power, egoism, and greed of the local elite constitute in his view by far the most important, if not the only, cause for the change.

While around 1600 many communities were indeed heavily indebted, and while the switch to indirect taxes that Caracciolo describes no doubt occurred in many towns and villages of the kingdom in this period, his view that it represented a universal pattern cannot be substantiated on the basis of the fifty or so cases he documents. The kingdom had then over 2,000 communities, and Caracciolo makes no attempt to convince his readers of why his sample should be taken as representative. In fact, his tendency to generalize and to oversimplify quite complex realities on the basis of limited evidence is one of the main flaws of his book: large and small communities, from prosperous and poor regions of the kingdom, with different economic structures, are all lumped together and made to fit one pattern. The interpretation is also weakened by Caracciolo's fixation on the faults of his ill-defined elite (his expressions "ceti abbienti"—and their opponents, the "poveri"—gloss over the complexities of early modern social structures) as the overall cause of the developments he studies.

It is indeed remarkable that Caracciolo chose to republish this book, unchanged but for a self-righteous preface, because these and other flaws in his work were pointed out long ago. G. Galasso had already labeled Caracciolo's 1963 article as "simplistic" (*Economia e società nella Calabria del Cinquecento* [2d ed., 1975], 355), and the first edition of the present book had been the object of intense criticism in Italy (see, for example, the reviews and Caracciolo's response in *Nuova Rivista Storica* [1986]). Besides generalizations on flimsy evidence, Caracciolo's work suffers from terminological confusion, scarcity of printed sources, lack of comparative perspective, and especially from his failure fully to consider alternative explanations for the pattern he observes. Little effort is made to relate fiscal practices with the demographic or economic structures of the various communities Caracciolo investigates, and he denies that the switch to indirect taxes, where it occurred, could also have been due to a changed economy in which real property was no longer the only source of wealth.

Although indirect taxes undoubtedly weighed heavily on the poor, Caracciolo fails to demonstrate

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that, in the situation of widespread elite tax evasion he himself documents, they did so more than other taxes. In addition, Caracciolo too easily assumes that the elite (whoever they exactly were, in all the different regions and communities of the kingdom) dominated local institutions, which was certainly not the case in many smaller communities. While its subject is an important and poorly known one, this book can therefore at best serve as a call to further research in the local finances and taxation of early modern southern Italy.

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ANTONIO CALABRIA. *The Cost of Empire: The Finances of the Kingdom of Naples in the Time of Spanish Rule.* (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1991. Pp. xvii, 179. $42.50.

In this excellent, if short and somewhat narrowly focused book, Antonio Calabria returns to a central question in the study of southern Italian history: how to account for the economic decline and the marked weakening of the state in seventeenth-century Naples. Why was it that, in the face of what some historians (but, it should be pointed out, not all) considered its striking degree of centralization and administrative efficiency in the fifteenth century, by the seventeenth the "state apparatus . . . lay in disarray or in shambles" (p. 2)? Calabria seeks his answers by exploring "connections between the economy, politics and society" (p. 4). He makes little effort here to dwell on the political realities of the sixteenth and seventeenth-century Neapolitan kingdom; nor does he devote much attention to broad economic trends. Rather, his attention is firmly fixed on fiscal politics, the social distribution of fiscal burdens, the economic consequences that these had on the population, and the mechanisms applied by Naples' Spanish overlords to raise the cash necessary for carrying out their imperial policies.

Calabria's contribution to the ongoing historiographic debate about the causes of the Neapolitan decline rests on his understanding that, by the middle of the sixteenth century, war and deficit financing, however important these may have been to the kingdom's consolidation in the late Middle Ages, were producing drastically counterproductive results. By the late sixteenth century a full-blown crisis was at work, produced by a conjuncture of reversals in agricultural production, a decline in silk trade, persistent mismanagement and fraud, and the inordinate weight of the fiscal burden imposed on Naples and its citizens. Calabria pursues this last point with considerable persistence, bringing to bear an array of evidence which, repeatedly, reinforces his conclusion that by the late sixteenth century Naples "was to see its tax burden multiply, its expenses rise well above its income, its debts skyrocket" (p. 36, a point reiterated on pp. 53, 58, 69, and 75).

In light of the clarity with which this fiscal crisis is described and the abundant and intelligently organized evidence brought to bear to buttress the author's contention that it was perhaps the determining factor in the reversal of Neapolitan fortunes, one reads almost in disbelief the book's conclusion, where it is stated that "through much of the sixteenth century, Naples . . . was probably taxed beneath its capacity to contribute" (p. 132). Whatever the meaning of this contention, in the body of his book Calabria assembles a vast array of evidence, presented in an eminently readable form, about fiscal structures, the sources of government income, and the government's outlays. The rise of the public debt, and its influence on the economy and on the investment strategies of private investors, represent themes that Calabria traces throughout, and to which he devotes the book's final and interesting if all too brief chapter.

One would like to know a great deal more than he tells us here (and on the basis of what he explains it appears that the available evidence is suitable for more intense and systematic analysis) about the operations of the market in government credits, the intensity of their turnover, and the degree to which they had become speculative commodities. One would also like to know more than the few tentative if highly suggestive bits offered about the role of the great foreign, mostly Genoese, financiers who often bankrolled the kingdom's government, but whose political and economic influence is pointed to only in a suggestive and allusive manner. In short, one can suggest that, following the publication of this excellent monograph, Calabria now return to the themes of this book and, in a more pondered and expansive manner, share with us his great knowledge of and his reflections on the "ruin" (p. 75) of the Neapolitan state in the early modern era.

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ROBERT C. DAVIS. *Shipbuilders of the Venetian Arsenal: Workers and Workplace in the Preindustrial City.* (Johns Hopkins University Studies in Historical and Political Science, 109th series, number 1.) Baltimore: Johns Hopkins University Press. 1991. Pp. x, 270.

In contrast to Florence, whose often tumultuous history was largely shaped by its guilds and workers, Venetian history seems to have been molded almost exclusively by its elites. Recently, however, this traditional image of the Republic has begun to change, as such scholars as Richard Mackenney, Ruth Martin, Dennis Romano, and Roberto Zago—drawing on the methods of social history—have sought to integrate the experiences of ordinary artisans, workers, and their families more fully into the story of Venice.

Robert C. Davis's study of shipbuilders in seven-

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teenth-century Venice is a splendid contribution to this new Venetian historiography. Focusing on the workforce at the Arsenal—a state-owned complex of shipyards and armories that covered nearly sixty acres at the eastern end of the city—Davis offers a rich, many-sided analysis of the lives of this privileged group of Venetian laborers. Davis begins with a meticulous examination of the composition of the workforce and its subjection to new forms of industrial discipline as the state sought to control costs through new, more rationalized forms of labor. Davis then explores the character of the neighborhood in which most of the *arsenalotti* (as these workers were known) resided. Finally he turns his attention to their broader civic and ceremonial roles in Venetian society.

The unifying theme in Davis's work—which should be of interest to both specialists in Italian studies and labor historians alike—is the relatively high degree of loyalty that the *arsenalotti* showed to the Venetian state and social order. Indeed, the Arsenal, an "institution that might have been a source of constant unrest in the city" was, in Davis's view, "a factor in its stability" (p. 177). This fact resulted to a large degree from the incorporation of the shipwrights into a wide range of civic and ceremonial roles. They served as the city's fire brigades, urban handymen, and guards to the doge. Especially in this latter role, they became symbols of worker loyalty to the republic even while they themselves enhanced their honor and fattened their purses through these supplementary activities. To some degree they were resented for their privileges by other workers in the city, but these tensions were released in the annual *battagliole sui ponti*, popular, ritualized clashes between the *arsenalotti* and other workers in carnivalesque but often violent struggles to win control of certain city bridges. Finally, the *arsenalotti* were, Davis suggests, less the victims of a new form of industrial discipline than clients of a baroque patronage system; they were more likely to benefit from individual petitions for assistance from their patrons and the state than from collective forms of action.

The analysis draws intelligently both on the vocabulary and paradigms of historical studies of the Industrial Revolution to illuminate an earlier, preindustrial world. Above all, this is a well-written and beautifully produced book, with not a few important lessons about some of the unexpected complexities of state capitalism in early modern Italy.

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AMEDEO LEPORE. *La questione meridionale prima dell'intervento straordinario*. Foreword by ROSARIO VILLARI. (Uomini e cose della nuova Italia, number 40.) Manduria, Italy: Piero Lacaita. 1991. Pp. 171. L. 20,000.

United Italy was not yet two decades old when in the 1870s the distinguished writers Pasquale Villari, Leopoldo Franchetti, and Sidney Sonnino raised the "Southern question" in pivotal works that spoke out on behalf of the underprivileged south against the overprivileged north. These so-called *meridionalisti*, as well as like-minded later scholars (notably Giustino Fortunato, Napoleone Colajanni, Maffeo Pantaleoni, and F. S. Nitti), argued that the former Neapolitan provinces of the new kingdom were the principal victims of both the Risorgimento and subsequent state policy during the period of liberal Italy (1861–1922). Specifically, they demonstrated with massive statistical evidence that the financial, fiscal, economic, and tariff policies of the central government of the new state systematically discriminated against the interests of the already impoverished Mezzogiorno while favoring those of the much richer and far more developed north. The consequence was to condemn the teeming peoples of the south to such misery-ridden lives that the only escape for millions among them was emigration to the Americas.

Amedeo Lepore's slender volume (its text is only 109 pages) on the impact of World War II and the immediate postwar reconstruction period on the Mezzogiorno and the existing pattern of north-south relationships takes the views of the *meridionalisti* as its point of departure. Indeed, the first third of the book is a summary of the southern question during the years 1861–1915 as it appeared in the works of leading *meridionalisti* as well as in those of radical writers like Antonio Gramsci and Emilio Sereni, whom Lepore clearly admires. Two short chapters on the Italian south under fascism and during the confused interim period of 1943–45 follow. In the first of these chapters Lepore shows how Benito Mussolini's regime continued liberal Italy's discriminatory policies toward the south. In the second chapter he focuses on the disastrous inflationary spiral set in motion by Anglo-American occupation authorities that further damaged the economic interests of the Mezzogiorno. There is nothing new in any of this.

Nor is there much new in the fourth and most substantive of Lepore's chapters, an analysis of the how Rome's economic reconstruction policies during the five years after 1945 ran counter to the interests of the south. American economic assistance under the Marshall Plan, for example, was disproportionately invested in the north, with only 13 percent of such funds actually finding their way into the Mezzogiorno. Neither was industrialization of the south encouraged while the neo-liberal financial policies of Finance Minister Luigi Einaudi drained resources from the region, deflated the local economy, generated unemployment, and provoked a wave of internal migration northward from the Mezzogiorno. In consequence, the already yawning gap that had long separated the north from the south in terms of social and economic development grew wider.

Lepore's conclusions are neither original nor sur-

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prising. His book is in fact disappointingly thin. It lacks political and ideological context and is not based on original research. Thus, it represents no advance on the existing scholarly literature in this field. It merely summarizes the already well-known published works of established scholars such as Pasquale Saraceno and Piero Barucci on postwar Italian reconstruction and its impact on the Mezzogiorno. That their various publications are cited fifty-nine and forty-five times respectively in Lepore's core fourth chapter is symptomatic of the generally derivative character of this all-too-slight monograph.

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ALBERTO AQUARONE. *Dopo Adua: Politica e amministrazione coloniale*. Foreword by LUDOVICA DE COURTEN. (Archivi di stato, number 14.) Rome: Ministero per i Beni Culturali e Ambientali. 1989. Pp. 422.

This publication is in honor of the late colonial historian Alberto Aquarone. The four essays gathered here deal with Italy's search for a coherent colonial policy after the debacle of Adowa in 1896. The second part of the book analyses the administration of Eritrea under the governance of Ferdinando Martini. The last segment of the volume is a synthesis of the debate on colonial policy and the foundation of the Italian Colonial Institute.

The defeat at Adowa produced a change in Italian colonial policy questioning whether Italy should remain in Eritrea. There were those who believed that the colony should be leased to King Leopold II of Belgium or rented to an Anglo-Italian society. Others proposed either to reduce the Italian presence to the Massawa-Asmara-Keren triangle or to completely abandon the colony. Hence when Martini arrived in 1898 the disputation over the future of Eritrea was unsolved. After the traumatic experience of Adowa, Italian colonial policy could be defined at best to be one of consolidation, implying that Italy would remain in Eritrea but give up its expansionistic policy at the expense of Ethiopia. The arrival of a civilian governor signaled that Italy would follow a policy of distension, creating good relations with the Eritrean people, preserving peace, and assuring Ethiopia that Italy did not plan to avenge the defeat at Adowa. Furthermore, Martini was given the task of fostering modest economic development of the colony by increasing gold production. The plan failed to meet expectations. Another aspect of development was agriculture, but Martini believed that Eritrea could become the home of only a limited number of Italian peasants. He warned that the whites would not have the monopoly over agricultural production and advocated that Italian farmers share the work and the wealth with Eritreans (p. 234). By 1902, however, Italian agricultural concessions were few.

A decade after the Battle of Adowa, therefore, Eritrea was not yet self-sufficient. It had to be sustained financially by the Italian government, which derived from its colony only defeat, sorrow, and shame (p. 71). Another possible justification for keeping Eritrea was that it could provide a home to Italian settlers, but instead emigration was directed toward the more hospitable countries of South America, where the Italians were able to maintain their national identity. Thus, the African possession on the Red Sea was not seen as necessary. The only other reason for keeping Eritrea was to increase trade while attempting the commercial infiltration of Ethiopia.

But before an Italian economic presence could be established in Ethiopia the Italian government had to settle new borders between Eritrea and Ethiopia following the Battle of Adowa. Ironically, in spite of Italy's defeat, the Mareb-Belesa-Muna frontier was strategically and politically favorable to Italy (p. 86). Martini's diplomacy must be credited, in part, for this situation. He was able to convince Emperor Menelik II that Italy did not have hostile plans against Ethiopia, and to offer generous compensation to the emperor for the upkeep of the Italian prisoners, which in reality was a war indemnity. In exchange Italy recognized the independence of Ethiopia and requested that Ethiopia not accept the protectorate of any other power, namely Russia. Menelik in turn favored the Italian presence at the Mareb River because he needed a friendly neighbor in the north, fearing an uprising of Ras Mangasha of Tigre. In this rivalry between Tigre and Shoa, Italy tried to be neutral, but realistically it had little choice because any other solution would require a strong military presence and a huge military expenditure, which Italy was not prepared to make. Italy's neutrality therefore was not static, and favored Menelik (p. 175) because the emperor had allowed Italy to keep the Mareb border. Italy in turn provided his troops with money and food during the Ethiopian campaign against Ras Mangasha. Furthermore, Italy, at Menelik's request, made a secret payment to Ras Mangasha of five million lire (p. 180). This way Italy insured a favorable border and maintained friendly relations with Menelik.

To further assure that Italy had no quarrel with Ethiopia, Martini introduced indirect rule in Eritrea (pp. 114, 121, 164), and he was instrumental in providing the colony in 1900 with a new constitution that established civilian rule with full military, civil, and economic powers. Respectful of Eritrean culture, the new governor, in a paternalistic fashion, did not impose Italian legislation on the Eritreans and allowed them to be governed according to their secular traditions. These traditions would be modified by Italian laws only when they clashed with "principles of civilization" (p. 240). Another innovation introduced by Martini was the creation, at the Ministry of Foreign Affairs in Rome, of a directorate for African affairs, allowing Martini to oversee the implementation of his reform program. In spite of the positive

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results of Martini's administration, Italian politicians and public opinion remained ignorant and indifferent about colonial matters, making it difficult for Italy to formulate a coherent colonial policy. Toward the end of his governorate, Martini encouraged the formation of several colonial societies. In 1905 the Colonial Congress at Asmara discussed a number of colonial problems and decided to establish the Italian Colonial Institute to help coordinate Italian overseas commerce, direct emigration to African possessions, and coordinate Italian colonial policy.

This book is meant for the specialist in colonial history. It is a mine of information and well researched, using all possible Italian secondary works and archival sources. While it does not make any major historical contributions, it is a serious attempt at analyzing Italian colonial policy after Adowa from Eurocentric viewpoint. The only drawback of this book is its redundancy, which leads to confusion. This flaw does not detract from the value of the articles, and serious readers will derive much benefit. This book points out once more that Italy, one of the colonial powers, has ignored the importance of its colonial history and lacks a reliable body of literature in this topic.

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DUCCIO BIGAZZI. *Il portello: Operai, tecnici e imprenditori all'Alfa-Romeo 1906–1926*. (Saggi di Storia, number 2.) Milan: Franco Angeli. 1988. Pp. 662. L. 48,000.

This exhaustively researched and documented study of Alfa Romeo at Milan in its formative years, which uses interviews with surviving militant workers as well as archival sources and published works, is heavily weighted in the direction of labor history. Nonetheless, Duccio Bigazzi has taken great pains to explore all of the sources bearing on financing and technology as well, and his work will interest historians of entrepreneurial capitalism. In this latter task he has exploited to the fullest extent a fortunate circumstance, the participation of the upper-middle-brow journalist and militant art critic Ugo Ojetti in the firm, as manager of his wife's capital. Ojetti's memoirs and observations uncover not only boardroom battles and financial expedients that punctuated Alfa Romeo's travailed existence but also offer occasional insights into the personalities of Nicola Romeo and other industrialists.

Certain central problems of Italian industrial development emerge from the author's extremely detailed narrative. One is clearly undercapitalization. Italian investors preferred government securities and real estate to the risks of advanced technology. Thus, investment banks or "mixed" banks had to be called on, and they soon became for all practical effects partners of the industrial engineering entrepreneurs to whom they extended long-term credit. Such commitments, however risky and unprofessional from a banker's standpoint, facilitated rapid expansion in times of booming demand, especially during World War I, but triggered a wave of bankruptcies and state-sponsored salvage operations during the depressions of 1920–24 and 1929–33. For Alfa Romeo these consequences were particularly painful in that it needed circulating capital in order to produce enough quality vehicles to secure economies of scale, and such ready capital, in turn, could only be forthcoming from its sales, especially abroad. From this vicious circle the firm could escape only through military purchases by the Italian state (especially of airplane motors), but such orders remained limited until the Ethiopian war. The firm, in spite of its technical excellence and the prestige gained by its models worldwide in various races, therefore continually teetered on the edge of a financial abyss.

In this connection, Bigazzi devotes much space to the firm's sidelines, such as locomotives, railway cars, and, especially during the war, munitions. But there the record is less brilliant than in motors: in the 1920s Alfa Romeo lost heavily in its efforts to produce electric locomotives of a non-standard type, and earlier, during the war, failed to produce according to government timing and specifications the famous Cerletti delayed-action shell fuses, the invention of a patriotic Italian psychiatrist who in later years discovered electric shock therapy for depressed patients.

The real core of the book is the intricate and sensitively worked-out history of Alfa Romeo's labor force. The skilled workers whom Bigazzi excels in dealing with present a complex picture. On the one side they are often militant socialists or communists, exemplary in their class consciousness, but that is far from the whole picture. They have an intense pride in their expertise and easily identify with the firm's successes in the world automobile industry.

Of especial interest is the failure of purely patriotic appeals during World War I. Neither the interventionist wing of Syndicalism nor the poetic manifestos of Gabriele D'Annunzio carried much weight among Alfa Romeo's work force, which engaged in agitation and even work stoppages in the midst of the conflict. Wartime inflation and threats to militarize factory labor probably had more effect in this sense than the purely political questions raised by Leninism. Bigazzi gives particular weight here to the emergence of women in the labor force.

The book presents many vignettes, some of them poignant as well as significant. The figure of the engineer-executive Pasquale Gallo, called in as successor to Nicola Romeo during the firm's restructuring in the mid–1920s, is especially memorable: he turned out to be a militant Mazzinian, active in assisting Republican political refugees, and finally, after arrest and an inconclusive trial, wound up as an Italian industrial representative in Brazil. After 1945 he again headed the firm's management. Sadder was the fate of the Communist "libertarian" militant

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Giuseppe Rimola. Fired as a subversive by Romeo, he eventually wound up in the Soviet Union, where in 1938 he was condemned to ten years penal servitude: he died obscurely during his confinement. Mussolini himself offers such anecdotal interest: he was often called on during the efforts to enlist the state as an investor of last resort and showed special awareness of the firm's needs. After Alfa Romeo had won world prestige in auto races, the Duce replaced his official Fiat with an Alfa Romeo.

Fascism, as a movement and political ideal, appears little in this study. The fascist blackshirt squads did indeed beat up and occasionally murder militant workers, but fascist organizers and agitators never penetrated the workshops; Alfa Romeo's management relied far more on the police and on its own purging of suspect workers, which was done by a retired army colonel. The rescue operations at Rome, imposed by the government on the Bank of Italy, have nothing specifically fascist about them. The conflicts between industrialists oriented toward development and contemptuous of cost accounting, and bankers concerned over liquidity, were resolved by the Italian state apparently on grounds of full employment rather than on any specifically fascist-futurist ideal of industrial titanism. Only with the Ethiopian War and the Axis did fascism come out as a prime mover in Italian economic decision making, but that period falls outside the purview of this work.

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GÜNTER SCHÖDL. *Kroatische Nationalpolitik und "Jugoslavenstvo": Studien zu nationaler Integration und regionaler Politik in Kroatien-Dalmatien am Beginn des 20. Jahrhunderts.* (Südosteuropäische Arbeiten, number 89.) Munich: R. Oldenbourg. 1990. Pp. 367. DM 98.

The question of the relationship between the nationalities of Yugoslavia has been an issue in the state's history since its formation in 1918. Neither the interwar policies of King Alexander, which stressed unitarism, nor Marshal Tito's socialist system were able to bridge the historic, political, economic, social, religious, linguistic, and cultural differences that have divided the peoples of this area for centuries. Understandably, therefore, their history has been a subject of keen interest to scholars. Within the last decade four outstanding books have been published in Germany, all of which add greatly to our understanding of Yugoslavia. They are Wolf D. Behschnitt, *Nationalismus bei Serben und Kroaten, 1830–1914* (1980); Wolfgang Kessler, *Politik, Kultur und Gesellschaft in Kroatien und Slawonien in der ersten Hälfte des 19. Jahrhunderts* (1981); Holm Sundhaussen, *Historische Statistik Serbiens 1834–1914* (1989); and the volume under review, by Günter Schödl. They should be required reading for anyone seriously interested in understanding the historical background of the current crisis in South Slavic history.

Schödl's book is divided into four chapters and a conclusion. In the first chapter he analyzes the evolution of Croatian national politics in the nineteenth century; the second chapter concerns the social changes in Dalmatia; the third examines the program of Croatian and Serbian political cooperation, known as the "New Course," which originated in Dalmatia and Croatia-Slavonia in the first decade of this century; and the fourth analyzes the relations of the resultant Croatian-Serbian coalition with the Habsburg and Hungarian governments up to the annexation of Bosnia-Herzegovina in 1908.

The first chapter is a succinct synthesis of the evolution of Croatian political thought beginning with the Illyrian movement of the 1830s and its transformation, after the revolutions of 1848–49, into Yugoslavism (Jugoslavenstvo); both concepts aimed at securing the unity of the South Slavs on the basis of political and cultural equality and were based on a romantic nationalist interpretation of their history. Beginning in the 1860s, however, Yugoslavism as a movement became less important than the growing Croatian national sentiment that supported the idea that Croatia and Zagreb should be the focus of South Slav politics and unity. Schödl then describes the challenge to any form of Yugoslavism offered by Ante Starčević's Party of Right, which denied the existence of separate Serbian and Slovenian nations, regarding both peoples as Croats, and Josip Frank's Party of Pure Right, the successor of Starčević's party, which acknowledged the existence of a Serbian people but denied that they had any political rights within the Triune Kingdom. By the turn of the century a new group, numerically small, composed of intellectuals and students, and shunned by the public at large, sought to revive the earlier concepts of South Slav cooperation and political unity through the reemphasis on the former concept of Yugoslavism.

Whereas the role that Dalmatia played in the Yugoslav movement has been largely ignored in most Western scholarship, Schödl describes the social and economic changes that occurred in the province and the influence that Dalmatian politicians had in the formation of the Croatian-Serbian Coalition of 1905. Without the understanding that these leaders reached in Dalmatia, especially after their strong differences following the Habsburg occupation of Bosnia-Herzegovina in 1878, the cooperation between the Serbian and Croatian parties in Croatia-Slavonia in the decade before the war would have been difficult to achieve. With the formation of the Coalition, the goal of the Croats was two-fold: first, to have the Serbian parties commit themselves to support the political and territorial integrity of the Triune Kingdom, and second, not to gravitate toward the independent Serbian state. In return the Croats would respect the political, cultural, and religious rights of the Serbs in the Croatian lands. As a result of

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this agreement, the Coalition remained the dominant South Slav political party in Croatia-Slavonia to 1914.

In line with recent research on the Yugoslav movement, Schödl comes to the conclusion that concepts of South Slavic solidarity were largely confined to the youth, which was vocal, but which certainly did not have wide influence among the population at large. In other words, "a homogeneous 'political culture' of 'Yugoslavism'" (p. 326) could not and did not emerge in Croatia. Anyone interested in the recent Yugoslav developments will find this book rewarding. It demonstrates the divergent views that Croatian political parties had of what their role should be in their own history and in Yugoslav affairs and it gives an indication of why many Serbian leaders both in Serbia and in the Croatian lands would develop reservations about any political union with the Croats.

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ALEKSA DJILAS. *The Contested Country: Yugoslav Unity and Communist Revolution, 1919–1953.* (Russian Research Center Studies, number 85; Harvard Historical Studies, number 108.) Cambridge: Harvard University Press. 1991. Pp. 259. $34.95.

This book has two major themes, first, the attitude of the Comintern and Yugoslav Communist Party (CPY) toward Yugoslav unity, and, second, the conflict between the Serbs and Croats, which Aleksa Djilas sees as "the most important single cause of the malfunctioning of the prewar Yugoslav political system" (p. 151). In the chapters on the first topic, Djilas discusses both the Comintern's view that the creation of the new Yugoslav state was both an imperialist and a Serbian victory. In 1925 a Comintern resolution called for the establishment of Croatia, Slovenia, and Macedonia as separate republics. In Yugoslavia, the small CPY, which was outlawed in 1921, took a more moderate position. Although strongly opposed to the political conditions of the time, the party, placing its emphasis on social antagonisms, expected that after a socialist victory the proletariat would forget their past national passions and become Yugoslavs. Support was also given to the idea of a Balkan confederation. The CPY was also the only Yugoslav party to which all of the nationalities contributed members.

After 1935, in line with its Popular Front policy, the Comintern shifted to the continued support of the principle of self-determination, but with the goal of a reorganization of Yugoslavia into a federation of autonomous, not independent, national units. In 1937 separate Slovenian and Croatian parties were organized. Djilas concludes that "Anti-Yugoslavism was never a dominant sentiment or a passionately felt cause of great importance to the party," even when that was its official policy (p. 102).

As far as the relations between the Serbs and the Croats are concerned, the book gives the background to the Yugoslav idea, which originated among the South Slavs of the Habsburg empire. The Serbs, in contrast, with a prewar state of their own, regarded the creation of the new organization in 1918 primarily as the unification of their own people. Faced by this attitude on the part of the dominant nationality, the Croats never felt at home in Yugoslavia. The author is extremely critical of the country's interwar regime, but he does not describe its failings in detail. He states that "the Serbs were not at all ideologically united . . . but a nation with a variety of political groups and ideas" (p. 82). These differences, unfortunately, are not made clear. In addition, a section on Serbia and its national movement, including a detailed analysis of the views of Ilija Garašanin, Vuk Karadžić, Nikola Pašić, and others on South Slav unity, would have contributed to the study. A separate chapter is devoted to the Ustasha movement and its Croatian background. The relationship of the Serbian and Croatian political parties between 1939 and 1945 is also discussed.

The Partisan victory and the constitution of 1946 allowed the implementation of the prewar Communist and Comintern programs. Faith was still placed on "the unity of the working people who would disregard nationality in the struggle for a social revolution" (p. 165). Marshal Tito, the author writes, believed that a Yugoslav nation could be created, "insisting that the borders of republics should not divide but unite" the Yugoslav people. By the 1950s the CPY regarded the national question as "completely solved" (p. 179).

This book, appearing at a time when CPY political control has survived in only two republics and when national animosities, in particular the Serb-Croat antagonism, have revived in a violent form, is obviously of importance for current developments. It should, however, be noted that the text, with its concentration on Comintern policy and on the Serb-Croat confrontation, tells the reader comparatively little about the Slovenes, Montenegrins, Macedonians, Albanians, and the other nationalities whose problems were just as complex and serious as those of the Serbs and Croats. A consideration of their position is necessary for an understanding of the Comintern program and its subsequent implementation by the postwar Communist regime. Djilas is a supporter of the continued unity of the Yugoslav state.

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JOHN O. CRANE and SYLVIA CRANE. *Czechoslovakia: Anvil of the Cold War.* Foreword by CORLISS LAMONT. New York: Praeger. 1991. Pp. xxvi, 352. $45.00.

This book presents the history of Czechoslovakia in the context of domestic politics and European power

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struggles from the country's birth as an independent state in 1918 to the Communist accession to power in 1948. Its principal author, John O. Crane, belonged to a family of intimates of Thomas G. Masaryk, a founder of the state and its president for seventeen years. Crane's father, a successful businessman, was Masaryk's patron, an adviser to Woodrow Wilson, and U.S. minister to China. Crane's elder brother Richard was first U.S. minister in Prague; their sister Frances was once married to Thomas Masaryk's son Jan, a diplomat and later Czechoslovak foreign minister. In the 1920s, John Crane studied in Prague while serving as a secretary to President Masaryk. The book's co-author, Sylvia Crane, shared his acquaintanceships with the Masaryk family, with President Edvard Beneš and his wife Hana, and with others in the country's post–World War II liberal elite. After her husband's death in 1982, Sylvia Crane undertook the task of preparing the manuscript for publication.

This family background is provided not only because the authors dwell on it but also to explain my expectations on reading the work. But the book contains only glimmers of the Cranes' privileged access to important politicians and statesmen, and it fails to justify the use of "anvil" as applied to Czechoslovakia's Cold War role. In his congenial introduction, John Crane states his belief that the Cold War began with the Bolshevik Revolution, along lines propounded by D. F. Fleming and A. Fontaine, yet this motif is virtually invisible thereafter. It is even curiously undermined in a chapter titled "Cold War Beginnings (1946)." In its sweep through half a century, the book compresses the lesser-known 1920s and early 1930s into one chapter, while it devotes six chapters to the oft-told story of Germany's resurgence, Sudeten German separatism, the Runciman Mission, the Munich Agreement, and the Nazi occupation of March 1939 and its aftermath. The wartime resistance movement abroad is handled adequately in tandem with Czechoslovakia's fate during the war. More engrossing is the final chapter, "The Death of Jan Masaryk," in which the Cranes propound a convincing theory, based on interviews and personal knowledge, that Masaryk took his own life in the fatal plunge of March 10, 1948.

The book makes for awkward reading because of hackneyed phrases ("come home to roost," pp. 67, 73), inexact use of sources (George F. Kennan, p. 27, n. 6), and unwarranted hyperbole, as in describing Beneš: "Never before or since in history did a newly emerging state get a more highly qualified or experienced minister of foreign affairs, straightaway" (p. 60). Factual errors place Social Democrat Bohumír Šmeral "in the nationalist camp" (p. 3), shorten Emperor Franz Joseph's reign to 1849–1915 (p. 9), and by implication absolve Slovak President Jozef Tiso of complicity in the mass deportation of Slovak Jews (p. 224), among many others. Misspellings and incorrect diacritical marks abound. The authors quote profusely and often in petty detail from well-plumbed British and American diplomatic papers, but they offer only a handful of tantalizing items from John Crane's "private file" and the "Crane family archives." A bibliography of about 300 items omits important recent studies by F. G. Campbell, R. M. Smelser, O. Johnson, and B. Unterberger, to name a few, and lists only two works in German and none in the original Slovak or Czech. The book lacks the authorial knowledge and firm editorial review necessary to prevent such infelicities. It represents a missed opportunity and will mislead the unwary.

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JAN KOFMAN. *Lewiatan a podstawowe zagadnienia ekonomiczno-polityczne Drugiej Rzeczypospolitej: Z dziejów ideologii kół wielkokapitalistycznych w Polsce* [Leviathan and the Fundamental Economic and Political Problems of the Second Republic: On the History of the Ideology of Great Capitalists in Poland]. Warsaw: Państwowe Wydawnictwo Naukowe. 1986. Pp. 283. 280 Zł.

Late in 1919 the Central Alliance of Polish Industry, Mining, Commerce, and Finance was formed. It was pejoratively referred to as "Leviathan" by the socialists and the name remained. The twenty-nine original concerns grew steadily and, in 1932, a previously rival body representing mining and metallurgical interests of Silesia joined Leviathan. The result was the largest and most influential big business organization in interwar Poland. Nevertheless, Leviathan had neither the economic nor political influence suggested by its name. It is the principle achievement of Jan Kofman's outstanding monograph that the limitations of Leviathan's influence are clearly demonstrated.

Politically, Leviathan's sympathies were predominantly with the National Democratic Right. Indeed the organization's director, Andrzej Wierzbicki, was a prominent member of that party. From the outset, however, Leviathan also had strong affiliations with the conservatives. (These are two separate strains in Polish politics that Kofman should have distinguished more clearly.) Transcending such differences was Leviathan's commitment to stability regardless of political coloration and the need for working relations between business and government.

This explains both Leviathan's support and influence in the center-right coalition governments of the early 1920s and its rapid acceptance of the 1926 coup, which brought the Right's bête noire, Józef Piłsudski, to power. Indeed, the next several years were the zenith of Leviathan's influence. Both Piłsudski and the ideologues of capitalism wanted order and were critical of destabilizing political partisanship. Yet the onset of the depression brought Leviathan's dedication to economic liberalism into opposition with an increasingly interventionist government. The fact

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that the out-of-power political Right had adopted a populist critique of big capital left Leviathan without substantial political allies by the end of the 1930s.

Leviathan's influence in the Second Republic was structurally limited. Both the great landowning conservatives and the powerful peasant movement were unsympathetic to the business lobby. Moreover, Leviathan's attitude toward the state was complex, torn between support for protectionist tariffs and ideological opposition to interventionism. In fine, the weakness, vulnerability, and divisions of nascent big business in Poland could only produce a lobby with fundamental limitations.

Kofman has made a major contribution to the historiography of the Second Republic. He seeks to define the economic and political function and context of big business in Poland. Based on extensive archival research and thorough knowledge of published materials, Kofman's detailed work is a fine supplement to Wojciech Roszkowski's recent works on banking and the landed aristocracy in the interwar era. Kofman is able to controvert a generation of Polish economic historians, notably Zbigniew Landau, Jerzy Tomaszewski, and Marian Drozdowski, regarding such matters as the role of foreign capital in Poland and financial policy during the depression. Kofman's balanced and informed treatment of Leviathan has demonstrated that the study of interwar Poland has only begun.

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Stanisław Sierpowski. *Piłsudski w Genewie: Dyplomatyczne spory o Wilno w roku 1927* [Piłsudski in Geneva: Diplomatic Disputes over Vilnius in 1927]. (Prace Instytutu Zachodniego, number 53.) Poznań: Instytut Zachodni. 1990. Pp. 203.

One of the most often told legends in the history of Polish-Lithuanian relations concerns the meeting of Józef Piłsudski and Augustinas Voldemaras before the Council of the League of Nations in Geneva in December 1927. As the usual version goes, Piłsudski, irritated by Voldemaras's verbal maneuverings, finally challenged the Lithuanian: would it be war or peace? After Voldemaras had accepted "peace," Piłsudski supposedly declared his job was done, he was going home, and the diplomats could clean up the details.

Stanisław Sierpowski challenges that account, devoting most of his attention to an examination of Polish-Lithuanian relations in the 1920s leading up to that confrontation. Sierpowski argues that Piłsudski was too much of a diplomat and too careful a tactician to allow himself such an arrogant outburst of temper.

I must confess to having in my day accepted the traditional version, although I noted, "The accounts of the meeting vary widely" (*The Great Powers, Lithuania and the Vilna Question* [1966], 197). My basic source, however, was the memoirs of the German translator at the meeting (Paul Schmidt, *Statist auf diplomatischer Bühne* [1949]), the only account that tried to savor the atmosphere as opposed to recording the debate. Sierpowski does not cite that book, and therefore, on this point, I remain rather unconvinced.

The body of Sierpowski's work, as an account of the Polish-Lithuanian conflict and specifically the Vilna question, at times seems to wander far from the author's concern about Piłsudski's behavior. Nevertheless, as one reads it in the shadow of the revival of the Lithuanian state in 1991 and the new dimensions of Polish-Lithuanian relations, the work has a contemporary significance.

Sierpowski seems to represent a new generation of Polish historical thought. The question of the 1920s and 1930s as to whether the Lithuanian stand on the possession of Vilnius/Wilno/Vilna was a pretext for other Lithuanian concerns and intentions seems alien to him. His primary concern seems to be with Polish issues rather than with Lithuanian-bashing, and he does battle with the Piłsudski-bashing of the Polish historians of the 1950s and 1960s. This is primarily a study of Polish dynamics on the international scene.

The author's documentation illustrates both the possibilities and the limitations of East European historiography in the last decade. He uses Polish sources exhaustively and adds new documentation from the archives of the League of Nations in Geneva and the Italian Foreign Ministry in Rome. Yet he cites published German documents and apparently did not use materials in Paris or London. Furthermore, he seems unaware of the recent spate of books on related topics by Finnish historians, who have published their works in English and German. Nevertheless, his work is a welcome addition to the international literature on the diplomatic history of East Central Europe between the wars.

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Lawrence Goodwyn. *Breaking the Barrier: The Rise of Solidarity in Poland.* New York: Oxford University Press. 1991. Pp. xxx, 466. $27.95.

Roman Laba. *The Roots of Solidarity: A Political Sociology of Poland's Working-Class Democratization.* Princeton. Princeton University Press. 1991. Pp. xii, 247.

According to the authors of the two volumes under review, a myth commonly held in both the East and the West is that "Solidarity," the first truly independent workers movement in Communist Eastern Europe, was above all a brainchild of the Polish intellectuals rather than of the workers themselves. It is then the main purpose of both studies to destroy that myth and to accord the Polish blue-collar working class its historical due. Both writers make a truly heroic effort

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to downgrade the input of the intellectual community and to exalt the political acumen and organizational skills of Polish labor.

Criticism of the attitudes and behavior of Polish intellectuals under Communist rule is hardly new. In the past three decades or so there have been several perceptive studies of the intellectual milieu in Poland, which openly accused some of the leading members of that circle of selling out to the Communists for mostly material benefits, thus forsaking their historical role as carriers of Polish national traditions and guardians of cultural and patriotic values. Indeed, it can be argued that until the mid–1970s the historical record has not been kind to the intellectuals who remained largely submissive to the Communist rulers, except for a brief moment of glory in March 1968, when a handful of them rose to challenge the increasingly conservative policies of the Gomulka regime.

The same historical record shows, however, that it was the Polish workers who, between June 1956 and August 1988, on five separate occasions successfully confronted the Communist Party, forcing it to change its leaders and/or abandon policies opposed by the working class. Presumably, it was this sharp contrast between these two groups that motivated both authors to produce their studies. I wish I could say that their efforts were crowned with success. Alas, this is not the case.

Roman Laba's study represents his doctoral dissertation, the research for which was started in Poland in 1980, at the time of the first wave of strikes that led to the creation of Solidarity. Laba was fortunate enough to acquire a substantial volume of unique interview material and at some point was even arrested by Polish authorities for his involvement with Solidarity. Hence, there is no doubt that his study is largely based on original primary sources, for which he deserves full credit.

The problem is that our expectations, aroused by the author's intimate knowledge of the events of 1970 and 1980–81, supplemented by a wealth of original materials, are simply not fulfilled. His overall conclusion, wherein he simply questions the validity of the interpretation of these events provided by Polish and Western intellectuals, must strike one as very modest indeed. In a sense, a mountain produced a mouse.

Lawrence Goodwyn's study is an entirely different affair. The author, identified as a "specialist in social movements" and author of the "definitive study of the American Populist movement," is honest enough to admit that at the time of Solidarity's birth in 1980 he had neither interest nor expertise in matters related to Poland, and did not know Polish. It certainly shows, and right from the start this lack of knowledge badly affects the quality of his study.

In contrast to Laba's painstaking analysis, Goodwyn's study is based in large measure on secondary and tertiary works that, paradoxically, he frequently proclaims as being biased and uninformed. His long book (390 pages of text, fifty pages of notes, plus a rather strange and convoluted concluding essay blasting "scholarship on Poland") is a good example of intellectual arrogance practiced by some members of the American academic community. Just because Goodwyn is an authority on U.S. social movements, he apparently fancies himself to be an instant expert on the Polish workers' movement. His study is an excellent testimony to the truth that overnight expertise often leads to embarrassing results.

Space limitations prevent a detailed discussion of the many questionable assertions and conclusions Goodwyn reaches. Suffice it to say that a lion's share of his examination of different cases of Polish workers' upheavals covers ground long made familiar by others and as such has little to contribute to our knowledge of these events. This in itself can be shrugged off as still another example of "trivial scholarship" that has recently become a focus of debate in the United States. In order to fortify his doubtful thesis, however, Goodwyn regularly launches rather furious *ad hominem* attacks on several highly respected Polish scholars and political leaders, branding them as prejudiced knaves who tried to lead the workers down the garden path. Such personal attacks do not really belong in what the author presumably considers to be a scholarly volume.

To conclude, all this means is that despite the appearance of the two volumes, the Polish worker movements, including Solidarity, still await historians able to examine them objectively. These two studies certainly do not fill the gap.

There is nothing wrong with trying to restore the balance between the respective roles played by the workers and intellectuals in Polish society. What is wrong is to present a highly idealized image of the Polish working class, ignoring its less than heroic behavior, as was the case in March 1968 when the Warsaw workers ended up beating Warsaw students protesting Communist government policies. This selective treatment simply will not do, and both authors are advised to keep in mind the fact that as in any society around the world, neither the intellectuals nor the workers represent a homogenous group but tend to be fragmented and divided among themselves. This obvious truth seems to have escaped both authors, which is clearly a pity.

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B. N. MIRONOV. *Russkii gorod v 1740–1860-e gody: Demograficheskoe, sotsial'noe i ekonomicheskoe razvitie* [The Russian City in the Years 1740–1860: Demographic, Social, and Economic Development]. Leningrad: Nauka. 1990. Pp. 271. 3 r. 70 k.

B. N. Mironov has provided the historian of urban Russia much to ponder and debate in this slim vol-

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ume on the evolution of the Russian city from the 1740s to the 1860s. Even those scholars who take issue with the analytic techniques employed or conclusions reached will welcome the wide array of data culled from archival sources and secondary works presented in the book's fifty-odd tables. As Mironov observes in his introduction, notwithstanding the sizeable literature devoted to Russian urban history (about 500 specialized studies according to his tally), there are still no generally accepted functional criteria for distinguishing urban from rural settlements during the eighteenth and nineteenth centuries and little consensus as to how to classify Russian urban centers according to socioeconomic criteria. Mironov sets himself the sizeable task of tackling both research issues. His first five chapters systematically address the number and distribution of cities in Russia during this period, the demographic dimensions of the urban growth process, the generic features of the urban social structure and how they change over time, both vertical and horizontal social change or mobility as reflected in changes in membership in the various *sosloviia* (estates), and the changing structure of the urban economy.

Mironov of necessity covers some ground well-traveled by other researchers. Thus, the importance of institutional barriers to rural-urban migration, as reflected in the pervasive system of permits and passports, the high level of transience common among so many "urbanites" of all classes, and the importance of administrative rather than economic activities in the evolution of many places classified as urban are but a few of the many themes discussed. But in almost every case Mironov brings something new to the argument. In large measure this derives from his analysis of an extremely large data set that tracks in detail the evolution of several hundred urban places over more than one century. For example, from the mid-eighteenth to the mid-nineteenth centuries he shows that the main occupation shifts from agriculture (almost half the urban workforce at the turn of the eighteenth century) to industry. While in itself this transformation comes as little surprise, it reflects an analysis of changing urban economic structure according to a seven-fold typology of cities and embracing an urban system of 266 places in the mid-nineteenth century.

In each of the five chapters the research methodology is similarly structured. Some historians may balk at conclusions drawn from sophisticated quantitative analysis, such as the factor analysis of the comparative level of urban economic development in chapter five, because of concern over the reliability of the original statistical information. But one of the virtues of the book is the wealth of statistical information presented that other researchers may use for different—or similar—purposes.

Perhaps the most challenging argument for other historians is Mironov's contention that there is ample evidence of vertical social mobility. Thus, in Mironov's opinion, the notion advocated by some historians that the *sosloviia* were largely impermeable is incorrect. For the historian of urban Russia the argument in this chapter alone makes for stimulating reading and would warrant adding Mironov's book to one's personal library. Arduously researched, well-organized, succinctly written, and frequently provocative in conclusion, Mironov's book is a welcome and important addition to the existing literature.

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Valentin Boss. *Milton and the Rise of Russian Satanism.* Buffalo, N.Y.: University of Toronto Press. 1991. Pp. xxvi, 276. $60.00.

Valentin Boss's book is an expert study that proceeds firmly and at a measured pace from the first Russian acquaintance with Milton and his Satan toward the middle of the eighteenth century and then on to Mikhail Gorbachev and perestroika. Part 1, "The Satan of the Enlightenment," contains chapters on "Satan and the First Translation of *Paradise Lost*," "Introducing Milton's Satan to the Common Reader," "Monks and 'Pocket Poets': Publications," "Masonic Devils and the Light Within," and "Satan, Pugachev, and the French Revolution." Part 2, "Satan as Romantic and Marxist Idol," discusses "The Demonic Tradition from Zhukovsky to Pushkin," "Milton's Satan and Lermontov," "Banning and Reviving Satan," and "Satan as Anti-Imperialist." The last topic, perhaps not immediately obvious, refers to the new standing of Milton's Satan in the Brezhnev era as the leader and symbol of the liberation of mankind from imperialist oppression. The volume has also a preface, acknowledgements, a note on transliteration, a list of abbreviations, notes, a bibliographic note, a bibliography, two sections (altogether thirty-two pages) of fascinating illustrations, an excellent index, and three appendices (on Milton's interest in Russia; an oration in English concerning Milton's Satan from Lermontov's school discovered by the author; and a chronological distribution of Russian translations of Milton). The book is beautifully published and contains a minimum of misprints, a high standard quite rare these days. Only one improvement comes to mind: the publication of the numerous Russian passages in the original Cyrillic rather than in transliteration.

Much of the study is devoted to a precise translation and analysis of texts in three languages, Russian, English, and French (because much of Milton's works and related materials came to Russia, as in the case of other British writers, not directly but through French intermediaries), with occasional use of still other languages, notably German, and the author performs his task impeccably. He appears to have a native knowledge of and even a deep artistic feeling for all three. Boss seems similarly masterful in his treatment of the specifics of Russian and general European

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publication, questions of authorship, possible influence, and the like. Nor does he limit himself to detail. Rather he speculates, suggests, and interprets, at times quite broadly, in his relentless pursuit of Milton's satanism in Russia.

It is more difficult to criticize this book than to admire it. To be sure it is a difficult work, best recommended to readers who know well both their Milton and Russian literary history. Perhaps more seriously, I believe at times that the author should have written more for the sake of his own argument, not only of his readers' ignorance. Thus, while he refers appositely to many satans, he discusses to some effect only two, in addition, of course, to the Miltonian one that constitutes the book: Goethe's rival figure of Mephistopheles and the little devil or devils of popular lore, found in many places, from folktales to Pushkin's doodles. But what about the devil of Christianity, who is indispensable for the larger framework and picture of deviltry, Miltonian or otherwise? Possibly a knowledge of that devil may be assumed, but then exactly what is being assumed? Fortunately the book includes references to two more forthcoming studies by Boss of Milton in Russia, and they may well remove my weak complaint.

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THEODORE H. FRIEDGUT. *Iuzovka and Revolution.* Volume 1, *Life and Work in Russia's Donbass, 1869–1924.* (Studies of the Harriman Institute, Columbia University.) Princeton: Princeton University Press. 1989. Pp. xviii, 361. $45.00.

This is the first part of a two-volume work on the society and politics of the coal and steel-producing region of eastern Ukraine, site of strikes and protests against the Gorbachev regime in 1989 and 1991. More recently the region voted heavily for Ukrainian independence despite its large numbers of Russian residents, who began to settle there in the period of this study. It is the first comprehensive treatment of the area, based on extensive work with archival sources as well as published documents, and combining detailed accounts of the formation, structure, and conditions of the Iuzovka labor force with attention to the ethnic diversity of the region (typically overlooked in Soviet works on the Donbass). There are separate chapters on housing, sanitation and health care, education, and culture. Two lengthy chapters on the structure of the work force and on working conditions and organization complete the volume. The second volume will treat the politics of the region over the same period.

The history of the coal and iron-producing settlement of Iuzovka (present-day Donetsk) lies at the center of the study, and Theodore H. Friedgut gives an engrossing account of how the successful Welsh iron magnate, John James Hughes (1814–89), came to establish the industrial settlement named for him against nearly impossible odds. Although aided by the pro-industrial policy of the tsarist government, the effort required considerable ingenuity and determination to overcome the problems of unfriendly physical, social, economic, and bureaucratic environments. Most of the British technicians and skilled workers accompanying Hughes were quickly discouraged and returned home, and it was necessary for other reasons to train Russian workers and specialists in any case, but Hughes and his sons held on to their expanding and successful plant and company town until 1917.

As a social history of the Donbass, the book attempts to present an exhaustive description of the miners and metalworkers, detailing more completely than most recent Russian labor histories the external aspects of the living and working conditions, as treated by surviving prerevolutionary studies of the region. In fact, Friedgut deviates so little from these studies that his work assumes something of their positivist cast. Lengthy descriptive passages piling example on example are illustrated with an abundance—at times an excess—of quantitative data. There are detailed discussions, for instance, of the various types of substandard and squalid dwellings inhabited by the workers, the kinds of medical care available at different times and places, accident and disability compensation, technical education, and workers' diets. The abundant detail includes valuable descriptions of producers' and consumers' artels, policies and perceptions of holidays and the work year, and moving and informative passages on mine and foundry work. At the same time, memoir-based anecdotes and personal reactions are minimal, and with rare exceptions the meaning of Donbass life and work to the individuals involved is not reported.

The resulting lack of personality of Friedgut's Donbass population is in part due to conditions noted by the author, above all the slowness of a true sense of community to develop among workers in new settlements like Iuzovka. The lag in the growth of social cohesiveness is convincingly accounted for by the miserable housing Hughes and other industrialists provided their workers and by the ethnic mix of the area's major population groups: Ukrainian peasants, who refused to engage in mine and factory work, the consequent need to attract Russian migrant labor from more distant provinces, and the eminence of the Jewish trader and educated element, hated and victimized by the other groups but possessing a developed cultural and political life. A unique, uncentered community grew up around the mines and smelters, "within the Ukraine but not of it" (p. 331). Administratively, Iuzovka never came under either zemstvo or municipal self-governing statutes, but remained a company town in the personal possession of the Hughes family, which provided water and sewage

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services, worker housing, education, a police force, and a public health commission.

The author's determination to exclude political conflict from this volume also contributes to the faceless quality of the workers, whose living and working conditions are so carefully described. Discussions of strikes, protests, and pogroms, in the 1905 and 1917 periods and earlier, have been postponed to the second volume. In addition, this volume practically excludes discussion of any developments after 1917, despite the closing date given in the title. Questionable though this Braudelian postponement of political developments to the last volume may be, it is downright regrettable that the list of postponed topics includes any summary discussion of labor-management relations which might have illuminated the lengthy descriptions of their separate components (wages, hours, fines, accidents, and so on).

We learn of the Hughes family's total control of Iuzovka, its domination of housing and the police, its draconian fines governing worker behavior and punctuality, and the primitive and dangerous conditions of the mines. But this is related piecemeal and never drawn together into a composite assessment of the company town's impact on its workers. It is also balanced by often favorable comparisons of the Hughes's operations with those of other Donbass mine and factory owners. Friedgut tells us that the better-off workers of Iuzovka were so appreciative of the steady work they received that "in times of political and social crisis they rally to the factory's defence, ready to do battle with anyone threatening it" (p. 331).

Did Iuzovka then represent an exception in the strike movement that swept southern Russia in 1903, prefiguring the great strikes of 1905? Did its skilled workers not play a leading role in politicizing the labor movement, as they did almost everywhere else in prerevolutionary Russia? Was the anticapitalist thrust of the Donbass labor movement perhaps blunted by diversion into anti-Jewish pogroms? The absence of answers to these questions, which Friedgut intends to pose in the second volume, means that readers of this book should be prepared for an incomplete experience, a kind of *cogitatio interrupta*. One may want to wait and read the two volumes together, therefore, even though separate parts of this richly descriptive work will not only stand on their own but are the only place to look for information on Iuzovka and the Donbass.

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STEVEN G. MARKS. *Road to Power: The Trans-Siberian Railroad and the Colonization of Asian Russia, 1850–1917*. Ithaca: Cornell University Press. 1991. Pp. xxi, 240. $31.95.

Steven G. Marks wrote this monograph as a corrective to the image of the Trans-Siberian Railroad as one of the great economic enterprises of the nineteenth century. Marks makes clear from the outset that political concerns provided the original impetus for the Russian autocracy to launch the colossal undertaking. There were recurrent fears in the 1870s and 1880s that a combination of Chinese and Korean migration into the region and the growth of indigenous regional separatism threatened to break the ties with the center. Beginning in the 1860s and rapidly gaining momentum in the mid–1870s a lengthy debate raged over the purpose and trace of a transcontinental railroad to the Pacific.

The range and intensity of public controversy over economic development in nineteenth-century Russia may still come as something of a surprise to nonspecialists. In this case the debate followed familiar lines. The engineers of the ministry of transportation, inspired by professional pride and semi-utopian visions, confronted the ministry of finance, which was skeptical of expensive state-built projects. The discussions spilled over into the mass press and public organizations such as the Russian Technological Society. Local interests faced by prospects of sudden growth or decline vied with one another over the direction of the line. Marks correctly portrays Sergei Iulievich Witte as the slavophile developer who fused the politics of empire with economic nationalism in order to win over Alexander III to his grand scheme. He cleared out the bureaucratic opposition, placed his own men in key positions, and overrode the regional protests of the Siberians who preferred gradual development. He also courted the heir and future tsar, Nicholas II, by evoking Russia's Asiatic destiny. Witte's brusque administrative style and his creation of the powerful Committee on Siberian Railroads as a planning agency gave the enterprise a grandiose sweep. But colonization and russification took precedence over economic development.

Marks accurately records the bureaucratic infighting, poor management, hasty and often sloppy construction, and sheer physical obstacles of the hostile terrain that made the Trans-Siberian "the most expensive peacetime undertaking in modern Russian history up to that time" (p. 217). Moreover, he argues that the railroad failed to fulfill one of its main functions, to defend the empire, during the Russo-Japanese War. All this is done in a careful, scholarly, and persuasive manner. He did not have access to all the relevant archives, but he has enough to make a strong case. Colonization gets short shrift, but that is in itself an enormous subject. More debatable is his conclusion. He sets aside much of his own evidence on financial and geographical problems and attributes the unsatisfactory performance of the line to politics, that is, to the role of the state as entrepreneur. He takes issue with economic historians who have identified the state as a dynamic substitute for the weak bourgeoisie, and he ends up by discrediting

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the enterprise as a forerunner of misguided Soviet planning. This may be so, but to prove it would require at the least a more comparative analysis of private versus public construction in Russia, a study of the comparative advantages of locally built lines, and the long-term impact on the development of the Urals and western Siberia. Finally, it is the political rationale for the railroad to which we must return. It is hard to imagine what would have been the outcome of either the Russo-Japanese War or the Russian Civil War if the Trans-Siberian had not been built. A definitive judgment on the value of the railroad remains an open question.

ALFRED J. RIEBER
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R. A. MIROVITSKAIA. *Sovetskii Soiuz v strategii Gomin'dana (20–30-e gody)* [The Soviet Union in Guomindang Strategy of the 1920s and 1930s]. Moscow: Nauka. 1990. Pp. 234. 2 r. 20 k.

The failure of the August 1991 coup in Moscow and the great changes that have swept Eastern Europe and the Soviet Union since 1989 have engendered hopes for progress in the formerly communist world. In the scholarly community many have hoped that glasnost and *novoe myshlenie* would enable Soviet scholars to throw off the shackles of Marxist orthodoxy and Soviet partisanship for a new, more open and critical stance inquiring into vital issues of international relations.

Initially, R. A. Mirovitskaia's account of Sino-Soviet relations from 1921 until 1937 seems to offer credence to this view. She offers a somewhat more nuanced and critical view than heretofore has been offered by her colleagues. She recognizes the strategic role of individuals in policy making in the Chinese Revolution. Mirovitskaia provides detailed information about the extensive Soviet involvement in China, especially during the Sun Yat-sen era. The detailed information she provides is the most satisfying part of the book. Mirovitskaia discusses the role of Vasilii Bliukher and Mikhail Borodin, their semi-independence from Moscow, the weakness of the Chinese Communist Party (CCP), and the supply of thousands of weapons and tens of millions of rubles to the fledgling government in Canton. She emphasizes the aid of Soviet advisers in preparing the new Chinese government's plans for the northern expedition (1926–27), training officers at Whampoa Military Academy, and helping to create the new Chinese army. Beyond this period, there is little new and much old in her recounting of the 1930s, of the struggles within the Guomindang in the period leading up to the Sino-Japanese War of 1937.

The "old thinking" suffuses the book. The old is ever with us: numerous obligatory quotes from Lenin (and Marx), a "liberated" Mongolia, the "great" and ever-positive role of the Comintern, the Soviet Union as ever right and never failing. There are numerous omissions of important people and events: the role of Mao Zedong, the Stalin-Trotsky debates, the 1927 Soviet failure in China, the massive decimation of the CCP, the Returned Students Clique, and the Long March. "Facts" are sometimes wrong. There were five (not two) extermination campaigns against the Chinese Communists, the CCP did not rule "enormous" territories in 1931, China was hardly a "totalitarian" state in this period, and so on.

Overall, Mirovitskaia has made some contribution with regard to the Sun Yat-sen era and the rise of Chiang Kai-shek. Let us hope that it will not take forty years of a veritable "generation of the wilderness" before Soviet social science begins to approach the level of Soviet science.

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MARCELLO FLORES. *L'immagine dell'URSS: L'Occidente e la Russia di Stalin (1927–1956).* (La Cultura.) Milan: Saggiatore. 1990. Pp. vi, 434. L. 60,000.

This book deals with an important topic, and within the limits Marcello Flores has set for himself, it works. Although the title promises a general discussion of the Soviet Union's "image" in the West, the volume deals primarily with how Western intellectuals received the Russian Revolution and the many vicissitudes of Soviet policy up to 1956. Flores is scrupulous in tracing the major twists and turns of Soviet history—Leninism, Stalinism, the "bolshevization" of the international movement, the five-year plans, the show trials, the Nazi-Soviet Pact, the antifascist struggle, the Cold War—and in documenting the position of important artists and thinkers. The intellectuals he discusses include but are not limited to Isadora Duncan, H. G. Wells, John Reed, and Theodore Dreiser.

The reader who wants to learn in what specific manner the Russian Revolution fascinated Western intellectuals, which groups first criticized Lenin, the reaction of American Stalinists to the attraction which American writers had for the USSR during the Depression, the results of Stalin's campaign against Trotsky—in short, the reaction of specific intellectuals to specific events—will find this book valuable. Flores's minute tracing of the opinions of major Western writers is no mean accomplishment and provides evidence of a great deal of research in the area.

Despite this core strength, the book does have some weaknesses. The first is conception. With some limited exceptions, the work is almost exclusively concerned with intellectuals. I would appreciate an analysis of how different groups and their institutions reacted to the events discussed by the author. The book is more about intellectuals than "image," as is confirmed by the sources.

Perhaps because of this weakness more than any

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inherent problems with the material, Flores seems unable to make many general observations. This is a pity because he does offer some interesting ones that could be further developed. He states, for example, that as 1917 receded, different countries tended to develop "national" perspectives on the Soviet Union—"political" in England, "cultural" in France, "ideological" in Italy, and "business" in the United States. But this line of thought is not followed through.

The same is true when analyzing particular individuals. Flores summarizes the positions of various intellectuals at the First Congress of Writers in Defence of Culture held in Paris in June 1935 (pp. 240–41). He quotes the particularly suggestive criticisms of communism and of communist interpretations of fascism made by the eminent historian and antifascist exile Gaetano Salvemini, but he is uninterested in these observations. Besides Salvemini, Flores could have chosen to develop the critical views of many other personages he discusses. He shies away from doing so, but this technique would have lent greater depth to his study. This is not a minor point because one of the crucially relevant questions regarding the Soviet Union's "image" is why the country continued to command such prestige among the Left in the West despite the atrocities that its leaders committed. Were the reasons primarily organizational or ideological? Here one not only is at a loss to determine what Flores believes but also comes away from the book with the uneasy feeling that any guesses would be bad ones.

A surprising lack in the book is the scarce attention paid to Italian intellectuals and to the Italian Communist Party, especially considering how attentive Flores is to American writers, for example. It is perhaps in Italy after World War II that the question of the Soviet "image" in the terms discussed above may in fact yield the most interesting conclusions. Furthermore, in this case and in general, the book would have benefited from a later ending date.

The book is well produced, although it lacks an index. And scholars will no doubt sympathize with the criticisms of research institutions that the author makes in his introduction.

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NEAR EAST

Aron Rodrigue. *French Jews, Turkish Jews: The Alliance Israélite Universelle and the Politics of Jewish Schooling in Turkey, 1860–1925*. (The Modern Jewish Experience.) Bloomington: Indiana University Press. 1990. Pp. xiv, 234. $27.50.

Aron Rodrigue has condensed in this relatively slender volume—166 pages of text divided into seven chapters—a wealth of well-researched and admirably analyzed information dealing with the emancipation of Ottoman Jews in the nineteenth century. The history of the Ottoman Jews has been the subject of several important studies. Rodrigue's work ranks among those at the top, if it is not at the very top, of this list of publications because it treats the Jewish emancipation within the larger framework of the Enlightenment, the Ottoman-Turkish reform movement, and the history of the Jews themselves in the nineteenth century. The treatment is precise, concise, and written in a simple but elegant style.

The Ottoman empire inherited in the Balkans the Roman (*Romaiote*) Jews who had lived there for centuries. The Spanish Jews, known as Sephardim (from Hebrew *sepharadh*—Spain), fleeing the Inquisition, joined their coreligionists there beginning in 1492 and became the dominant group until replaced by the East European Jews, or Ashkenazim, mostly after 1880. The reform movement in the Ottoman state, which began early in the nineteenth century, affected the Jews rather late (the 1850s) despite their historical ties with Western Europe, and only after a series of visits by westernized Jews had made others aware of the economic and intellectual plight of the Ottoman Jews. These Jews had remained far behind the Greeks, Armenians, and even Christian Arabs, who received support from England, France, and Russia, the powers contending for influence in the Middle East. Although England opened a consulate in Jerusalem in 1839 and offered protection to the Jews, the British interest stemmed as much from the need for a loyal constituency to oppose the French and the Russians, who manipulated the Catholics and the Orthodox Christians to further their interests in the Christian Holy Lands, as from genuine concern for Jewish welfare. In the end, the Jews had to take care of themselves.

It was not, however, the Ashkenazim or the French or German Jews who initiated the movement for the "regeneration of the Jews of the Ottoman Empire in general and those in Palestine in particular by the introduction of modern schooling, modern trades and agriculture" (p. 17). Rather it was the "Francos," that is, the Italian Jews, an international commercial group that, seeking to reinvigorate commercial ties with the West (monopolized by Greeks), led the drive to introduce European-type education for Ottoman Jews. The task of establishing a Western style of education was undertaken by the Alliance Israélite Universelle (AI). Its goal was to transform the Ottoman Jew, still immersed in the medieval world, into a new type of human being. This aim was born out of a feeling of "Religious fraternity that has been bequeathed to us by thirty centuries of glory and suffering" (p. 18). According to Rodrigue, the moving force behind the Jewish emancipation was the AI, which provided the teachers, finances, and leadership necessary to the functioning of the modern schools. Thus, by the time of the Balkan War (1912–13), practically every major Judeo-Spanish community in

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the Ottoman state had its Alliance schools, enrolling the equivalent of about 40 percent of school-age children (p. 92).

The educational emancipation of the Ottoman Jews was aided by the reform movement in the Ottoman state, which granted equality to non-Muslims, by the Turks' own interest in Western education, and by the excellent Turkish relations with France, which was regarded as the "representative" of Western civilization. Yet the most important factor in the Jewish educational emancipation was the lack of any Ottoman "national" education policy. The noninterference of the government in education permitted each ethno-religious community, including the Jews, to select and implement freely a school system of its own choice. Rodrigue demonstrates that the AI schools, although teaching in French and following the French educational philosophy and ideals, were not controlled by Paris, at least not at the beginning, although they were instrumental in disseminating French language and culture and thus strengthening French influence in the Middle East. The AI schools embraced the idea of emancipation through enlightenment, but in practice they imbued their pupils with the philosophical underpinnings that made the idea of the nation-state seem the natural form of political organization. In the end the AI schools fell victim to Jewish nationalism, which they had inadvertently nurtured in the name of introducing French culture and language. After the turn of the century the Zionists, claiming to be the true representatives of the Jewish community, challenged the AI schools and their programs. Even the belated intervention of the French government (which initially considered Zionism a political utopianism encouraged by Germany for the purpose of opening a German foothold in the Middle East) on behalf of the AI could not ultimately forestall the Zionist victory in securing control of the schools. In turn, the rise of the Turkish national state after 1923 and the educational reform of 1924 forced the Jewish schools to "nationalize," in effect to pay the Zionists with the same currency they used to gain control of the AI schools. By this time, as Rodrigue indicates, the AI schools had fulfilled their initial mission in bringing the Oriental Jews into the modern age.

One wishes that Rodrigue had given additional space to the Ottoman-Turkish sociopolitical frame in which the Jewish educational emancipation took place. He could have stressed the fact that non-Jews also could attend AI schools. These are, however, minor gaps in an excellent piece of scholarship that provides deep insights into the history of the Middle Eastern Jews in particular and the Ottoman transformation in general.

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JEFF HALPER. *Between Redemption and Revival: The Jewish Yishuv of Jerusalem in the Nineteenth Century.* Boulder, Colo.: Westview. 1991. Pp. xiii, 290. $35.00.

Through a detailed study of personalities, Jeff Halper focuses on the growth and changing character of the Jewish community of Jerusalem in the nineteenth century. Many of the controversies of modernity engulfing the European Jewish world were reflected in the life of the Jerusalem community: between Hasids and their opponents (*misnagdis*), between the *Prushis* (the ultraorthodox who reconciled their differences with the Hasids) and those who advocated secular Jewish education (*maskilim*), and between Zionists and anti-Zionists. Jerusalem, however, was a bastion of ultraorthodoxy in the nineteenth century, where most of its Ashkenazi residents, many of whom were supported by contributions from the Diaspora (known as *haluka*), awaited divine redemption rather than a national revival advocated by the Zionists. It is because of the disparaging attitude toward the "Old *Yishuv*," the term that distinguished the pre-Zionist Jewish settlement in Palestine from the Zionist immigrants of the so-called "New *Yishuv*," that the Jewish community of nineteenth-century Jerusalem, in the author's opinion, "is probably the least known of any major Jewish community of the modern period" (p. 227). Although this is a questionable assertion, the author is certainly correct in pointing out how Zionist historiography has downplayed the importance and distorted the reality of this community for ideological reasons.

The book represents a synthesis of the numerous articles of a newer generation of Israeli historians writing in Hebrew on "local" history for a "local" audience. Combining these studies with memoirs of the principal actors appearing in the story, Halper weaves a rather dense historical narrative, interlaced with detailed and entertaining anecdotes and vignettes. Of his cast of characters, besides Eliezer Ben Yehuda, most would be unknown to all but the most specialized of scholars. But it is through the controversies of these individuals that Halper traces the evolution of the Jerusalem community. He writes of a highly contentious community, separated into small corporate groups based on religious or ethnic differences. The Jewish population grew significantly in the nineteenth and early twentieth centuries, although few immigrants to Jerusalem were among the secular Zionists who began settling the coastal plains of Palestine from the 1880s. Nonetheless, the impact of the growing and more dominant New *Yishuv* was felt in Jerusalem, as even some of the ultraorthodox began to accommodate themselves to the changes, accepting the Zionist ideals of economic self-sufficiency, especially since the *haluka* funds were diminishing. The Sephardi population of the Old *Yishuv* was not only more accommodating to these changes but also contributed to the emerging national culture, for unlike the Ashkenazim, the Sephardim saw no

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great dichotomy between modernity and orthodoxy. The study is somewhat imbalanced since the Sephardim, who constituted nearly half of the Jewish community, receive far less attention than the Ashkenazim, and are presented in a somewhat static manner.

While the themes of the book are clearly narrated, the methods and general contextual framework of the study are problematic. Halper, an anthropologist, purports to write a historical ethnography, distinguishing his study from social history by his simple statement that he interviewed people who were still tied to a world view of past times. How these interviews enter into his narrative is never revealed. The text reads not as a historical ethnography but as conventional social history. For the historian, the book is disappointing because archival sources are not used. In the introduction, Halper invokes various proponents of "new history," and Clifford Geertz's notion of social activities as "texts," but nowhere are these methodological or theoretical issues confronted in the book. The various anecdotes used in the narrative are often taken at face value, and the *mentalité* of the actors is often presented stereotypically rather than multileveled and nuanced, as in the Geertzian sense. The result is a history that remains parochial. The main contribution of this book is not in the originality of research, nor in its theoretical insights, but in bringing together in English material that would otherwise be inaccessible to a non-Hebrew reading audience.

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DALIA OFER. *Escaping the Holocaust: Illegal Immigration to the Land of Israel, 1939–1944*. (Studies in Jewish History.) New York: Oxford University Press. 1990. Pp. xiii, 408. $35.00.

DINA PORAT. *The Blue and the Yellow Stars of David: The Zionist Leadership in Palestine and the Holocaust, 1939–1945*. Cambridge: Harvard University Press. 1990. Pp. x, 334. $27.95.

The core of every historical account of Zionist policy and action in the years preceding World War II, and of Zionist activities during the course of war itself, seeks not only to give a full and accurate account of the historical events or an improved understanding of what really took place during this dark and monstrous period but also to pass historical and moral judgment, and to condemn the accused. Anyone judging events would indeed have to face a series of crucial questions, such as whether Zionist leadership could have foreseen the coming storm and its aftermath, or whether they could have more efficiently helped Jews reach the safety of their "national home" in Palestine.

Such issues are indeed some of the most important to have been raised in Jewish history. They loom over the collective Jewish consciousness and existence, their ghosts lurking, reappearing and disappearing. It is small wonder, then, that these reflections of guilt and remorse are placed at the heart of a long and bitter controversy pertaining to the historiography of the Jewish people, and to the polemics of its collective memory. A continuing debate tries to clarify the answers to the most serious accusations, many of which reflect intensely emotional feelings and a priori ideological and political attitudes and beliefs. Any seriously critical or scholarly attempt to deal with these issues must therefore be welcomed as a major contribution. The two studies by Israeli scholars Dalia Ofer and Dina Porat are contributions that add considerable insight to our understanding of Jewish history during the period in question.

Some preliminary notes are required. Before reexamining a historical perspective, and prior to passing judgment, it is important to remember that an argument basic to Zionism was its declared commitment to the overall destiny of the Jewish people. At the same time, however, one must also remember that the Zionist movement was unable to acquire leadership of the Jewish people or to solve the so-called "Jewish Problem." As Porat rightly argues, Jews of the free world were never really prepared to accept Zionism as a leading movement, and before the war the Polish Jews were unable to amass a unified body to fight Polish anti-Semitism (see Emanuel Malzer's excellent study, *Political Strife in a Blind Alley: The Jews in Poland 1935–1939* [1982]). The idea that Zionism could act efficiently as the "Jewish Agent" involved in finding "real" territorial solutions to settling Jews in a land of their own, or organize Jewish immigration to one of the so-called "immigration states" (such as the United States), fails not only to see that doing so was to act against the basic raison d'être of Zionism but also overlooks the fact that no real territorial solution in fact existed. Furthermore, one must also remember that the majority of European Jewry was at best indifferent to declared Zionist objectives, and at worst anti-Zionist.

Those who blame Zionism are, to my mind, retroactively placing on its shoulders an unfair and altogether too heavy burden when they accuse it of not having provided for the safety of thousands of Jews; they conveniently forget that these multitudes never even believed in Zionism as a possible or even desirable solution. These same critics are also ignoring the inherent weakness of the *Yishuv*, its worldwide political demise during the 1930s (as today), and the fact that its existence was severely threatened at the time. Paradoxically, in the 1930s, there were many Zionists who believed that at this moment of desperation and crisis Zionism should have braced itself and sailed off into the storm, assuming political clout of its own in Europe, and ultimately using the international crisis to its own advantage by posing as an ally to what were in fact its staunch adversaries.

In reality, the keys to Palestine were in the sole

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possession of policy makers in London, not those in Warsaw, Bucharest, or Berlin. British policy was aimed at separating the Jewish question from the Palestinian question. After Great Britain retreated from the Partition Plan there was no way of swaying its interest in the Middle East nor its attitude toward Zionist self-fulfillment in Palestine by presenting Zionism as a solution to the internal problems of the Jews in Poland or Hungary. Indeed, Zionism was forced to act in keeping with "the Age of Dictators," struggling concurrently with anti-Zionist and anti-Jewish trends in Britain and the United States, as well as growing anti-Semitism throughout Central and Eastern Europe.

As there was no other alternative, Zionism was forced to seek the support of anti-Semitic or semi-anti-Semitic governments in order to secure its political status and freedom of activity. Ironically, therefore, it was anti-Semitism rather than philo-Semitism that became Zionism's ally. It is unfair to claim that Zionist leadership observed the flight of Europe's Jews from a cold and rational political viewpoint of self-interest, because the only viable mass-solution for Jewish immigration was not a humanitarian one but rather a political one. Zionist leaders could have objected to any existing non-Zionist solution but could not prevent the implementation of such a solution. Because no political solution was at hand, and Jews were trapped in Europe, Zionism could offer no more than partial solutions, effective for no more than a handful of people at a time. We should also not forget that there were not many people who predicted the advent of a world war before the end of 1939, and the Jews in Nazi Germany and Austria were subject to different conditions from those in Eastern Europe. The large population of Polish Jewry was assaulted by the Polish people and the government, yet no one foresaw the extent of this mass discrimination, let alone a Polish "final solution." The *Endlösung* could only have taken place as a result of Nazi occupation in Eastern Europe.

Ofer's book presents the best available account of the Illegal Immigration (*Aliya*) to Palestine during the years 1939–44. Between 1934 and 1944, sixty-nine ships carrying 38,542 Jewish immigrants and refugees arrived in Palestine. 1,399 were drowned or did not arrive (see the list on pages 323–25). One should distinguish between the period 1934–39 and the years of war itself. During the first period, illegal immigration was a means whereby Zionism could combat the stringent measures and restrictions imposed on the numbers of "certificates" distributed by the Mandatory Regime. It was, however, also a manifestation of inner strife and tension, and new active trends, emerging within the two major movements in Zionism: the labor movement and the revisionist movement. The main internal struggle took place between the active groups and the leadership of each movement. The leadership was anxious not to let illegal activities endanger its political endeavors and saw illegal *Aliya* chiefly as a political instrument that might cause British policy to change. For activists in both parties, illegal *Aliya* became a manifestation of their desire for activism. It became an integral part of their struggle with the British Mandate, and because the war was still far off, this activity was primarily a result of growing Jewish pressure to leave Europe (mainly Poland). Illegal immigration was thus not a response to leadership initiatives but rather part of a modern Jewish Odyssey, an expression of boldness and small-mindedness, private enterprise and political interests, bitter conflicts between factions, human suffering, and deliverance.

Greater organization could possibly have brought more Jews to Palestine during this time, but illegal immigration was not to become a central aspect of Zionist groups until 1939. The war changed all this drastically, and illegal immigration became the *Yishuv*'s main means of combatting both immigration restrictions and British policy. Britain did its best to stop this immigration both before and during the war (see Bernard Wasserstein's study, *Britain and the Jews of Europe 1939–1945* [1979]). Deteriorating war conditions in Eastern Europe, the Balkans, and at sea eventually became insurmountable, bringing an end to more aggressive performances by the Mosad for *Aliya Bet*. Ofer rightly claims that more could have been done during 1941 and 1942, but the outcome would have been limited, and only a handful of people might have been spared. She also claims that "There was no all-about effort to combat the indifference of the Western democracies, even though all were agreed that without their active involvement there could be no large-scale rescue" (pp. 316–17), but one wonders if such an effort could have brought about a change in the attitude of Western democracies. Illegal *Aliya* did not therefore stem from a desire to take direct and independent action but happened simply because there was no other choice.

The history of illegal *Aliya* consists primarily of the history of each individual ship. She successfully gives the reader a reliable picture without foregoing the dramatic details of human interest. Despite bringing to safety but a handful of Jews, and not really being able to redirect British policy, illegal *Aliya* signified a major shift in the attitude of Zionism toward European Jewry and the ensuing structure of Zionist activities. Only when the war ended did *Aliya* become a major instrument in the Zionist struggle against British policy and a major force in the creation of the state of Israel.

Porat's study touches on an even more sensitive subject, the attitude of the *Yishuv* and its activities during the war years. Here not only the issue of illegal immigration is at stake but also other matters such as historical interpretation, rescue operation funds, the Joint Rescue Committee, and rescue plans and operations such as the Ransom Plans and Military Plans. The stance of the *Yishuv* and its behavior should be considered in the light of the way the

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Jewish community continued to live a normal life during the war, both before and after details concerning the mass-murder of Jews and the "final solution" became common knowledge, and in response to the fate of the Jews and rescue operation plans; and whether the *Yishuv* did all it could to employ its resources and authority in order to effect change in British policy and secure the rescue of as many Jews as possible. Moral judgment concerning ideological viewpoints should not be confused with the main issue at hand: could the *Yishuv* have done more had it been fully aware of the coming Holocaust and better equipped to deal with such a mission? As I have already claimed, it might perhaps have done a lot more for a few people, but hardly anything more for the masses. Porat has touched on an extremely sensitive chapter in Jewish history, fully aware of its being an open wound, and no less aware of her responsibility as a historian.

We should add that by concentrating on Zionism and the *Yishuv* we have neglected the role played by American Jewry during these years. This subject is also the focus of many studies and bitter debates. Nevertheless, these two studies present us with central aspects of the whole picture. It is altogether too easy to accuse them of being overly moderate in their critical stances and even in trying to justify or condone the *Yishuv* and its leadership. But being an Israeli scholar, whether writing in Hebrew or English, does not mean that one should be expected to be a revisionist historian, intent on revealing a strong but somewhat blind Zionist strength, with an interest in exposing the indifference, passivity, and politically insensitive guilt caused by a terrible and unforgivable crime. The historical truth is far more complicated, far more full of collective human helplessness in the face of the most heinous crimes committed at a most terrible time for any people's leadership. Recent historical events have taught us that a state can indeed act as a messenger of salvation, or as an agency for large-scale immigration, but even then it needs large-scale resources and the support and cooperation of other great powers. The so-called use and misuse of the "lesson of the Holocaust" should not blur the fact that some historical and national goals can be accomplished only by a sovereign state, and that often even a state is not enough.

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AFRICA

NANCY ELIZABETH GALLAGHER. *Egypt's Other Wars: Epidemics and the Politics of Public Health.* (Contemporary Issues in the Middle East.) Syracuse, N.Y.: Syracuse University Press. 1990. Pp. xiii, 234. $32.50.

This book by Nancy Elizabeth Gallagher describes the spread of malaria, cholera, and typhus in Egypt during the 1940s and the political implications and decision making involved with questions of health. The author discusses the impact of the epidemics on Egypt's population, the various agencies involved in fighting the epidemics, the role of private and public American efforts in Egypt, and the role of the British in fighting the epidemics or hiding their existence, as the case may be. Altogether Gallagher demonstrates the significance of political interests to decisions regarding matters of health. Thus, she raises a number of significant questions and issues that are important for understanding the process of historical change. These include, for example, how health issues are manipulated by politicians; how political interests affect health policy; the nature of Egyptian interparty squabbles over power; the competition between the British and the Americans over influence in Egypt; and the persistent conflicts between the king, the British embassy, and the Egyptian government over who has paramount power in Egypt. Gallagher shows that the answers to these questions are never simple, and that the forgotten wars were more than a fight against the spread of epidemics and against poverty.

The sources used in this study are quite extensive and impressive. Through considerable research in American and British archives, Egyptian government and United Nations publications, as well as interviews, Gallagher succeeds in showing the intricacies of politics, thereby adding to our understanding of the relationship between state and society and the impact of one on the other. She also adds to our understanding of the way women have involved themselves in decision making and reveals the central role played by women in health politics in a society that is customarily portrayed as a patriarchy, where women are subjected to the power of men and where decision making is the prerogative of absolutist rulers.

One weakness of the work is the author's concentration on the contribution of elites, particularly elite women, to the treatment of epidemics. Epidemics such as typhus were not new for Egypt and local methods were relatively effective for centuries before the introduction of modern medicine. Besides, Gallagher does not appreciate the relative significance of health, disease, and class issues. Thus, the relationship between culture, social structure, and health is not sufficiently covered. For example, regarding cholera, since mosquitos do not discriminate, why is it that more of the poor died than the rich (if that was the real case)?

Notwithstanding these criticisms, this important book sets new directions for historical research, particularly regarding the Third World. It should be of general interest to social historians and required reading for those interested in the history of the Middle East, imperialism and dependency, history of medicine, and women's studies.

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RALPH ERBAR. *Ein "Platz an der Sonne"? Die Verwaltungs- und Wirtschaftsgeschichte der deutschen Kolonie Togo 1884–1914*. (Beiträge zur Kolonial- und Überseegeschichte, number 51.) Stuttgart: Franz Steiner. 1991. Pp. 350. DM 86.

Ralph Erbar's study, a 1990 Ph.D. dissertation, presents a useful survey of German colonial administration in Togo, the smallest of Wilhelmine Germany's African colonies. Unlike Arthur Knoll's earlier account (*Togo under Imperial Germany* [1978]), it is based on administrative and mission records in Lomé and other West African archives, as well as on a wide range of relevant source material in Germany.

The main themes are familiar to the student of early twentieth-century colonial history, especially West Africa's: the effort of a handful of colonial officials to establish their authority by bending indigenous rulers and political systems to their purposes; land and labor policy; economic and infrastructure development within the context of severely limited financial resources; relations and tensions between government officials and missionaries, especially over African education.

It is therefore regrettable that the author eschewed comparisons between Togo and neighboring colonial territories, especially the Gold Coast, to which Togo was closely linked by economic, cultural, ethnic, and historical ties. (He explicitly denies, probably rightly, the usefulness of comparisons between the Togolese experience and that of Germany's other African colonies.)

The main conclusions of the study are modest, and on the whole convincing. In the absence of a clear concept of colonial policy in Berlin, German administrators—district officials as well as colonial governors—had unusually large scope for individual initiative and judgment. Executive autocracy began to give way to a bureaucratic system and legal framework only gradually, in the last decade of German rule. Togo's reputation as Germany's "model colony" was not wholly undeserved, but must be relativized.

Unlike Tanganyika, Southwest Africa, and Cameroon, Togo did not experience large-scale African protest and military repression; there was some economic growth, based on peasant agriculture, and hence a degree of financial self-sufficiency. The administration successfully defended Africans' land rights against European speculators and plantation interests and evinced an increasing interest in public health and education. But economic development was halting, some efforts (for example, the cotton cultivation fad) were ill-conceived and ultimately unsuccessful, and a generally paternalistic native policy was marred by thoroughly racist preconceptions and administrative arbitrariness.

Erbar disclaims any intention to write African history, describing this work as administrative history: in effect the history of interactions between the few hundred Germans (colonial officials and interest groups in Berlin, administrators, missionaries, and traders in Togo) who were involved in the colony's affairs between 1884 and 1914. But even their intentions and actions cannot be fully understood without reference to the African setting and the interests, actions, and wills of the African population. The author himself provides a clear example. A controversy over language instruction in primary schools—the administration and Catholic missions preferred to introduce German as early as possible, the Protestant North German Mission supported native tongue instruction in the early grades—was essentially decided by the clear preference of African parents for the Catholic schools, which came to threaten the interests of the Protestant mission. Erbar's study thus cannot be seen as more than a useful contribution to the history of Togo under German rule.

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BENJAMIN C. RAY. *Myth, Ritual, and Kingship in Buganda*. New York: Oxford University Press. 1991. Pp. ix, 239. $37.50.

This is a good book on Buganda, although the title seems a bit too restricted as an indication of its contents. Benjamin C. Ray divides his book into seven chapters. The first analyzes the early ethnography of the nineteenth and early twentieth centuries. While noting the contributions of travelers, missionaries, and colonial officials, Ray chooses to focus on Sir Apolo Kaggwa, the first regent of the kingdom; Reverend John Roscoe, a Church Missionary Society missionary in Buganda; and on the influence of Sir James Frazer. As he states: "taken together, the writings of Roscoe and Kaggwa constituted a unique two-dimensional picture of late-nineteenth-century Buganda, indigenously described in Kaggwa's Luganda works and systematically presented in Roscoe's ethnography written according to Frazer's ethnographic and theoretical scheme" (p. 23). He provides a fascinating description of how the ethnographic knowledge of Buganda was produced. Frazer, who had never been to Africa, was the master thinker orienting Roscoe (and through him Kaggwa) and shaping his descriptions and interpretations of ritual practices (birth, marriage, death, and so on), ritual homicide, and kingship. In fact, *The Golden Bough* (1890) was the basis from which Buganda ethnography and anthropological knowledge sprang.

Ray presents in the next three chapters information on the mythological beginning of Buganda with Kintu, the origin of the kingship, and the signification of the king's body. The progression is exemplary: it goes "from Kintu–the patriarch to Kintu–the royal founder" (p. 74), or, in other words, from a reading of mythical narratives to historical interpretations about Buganda, and, in chapter 3, to the presentation of a theory of royal body symbolism.

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In the process of studying the Buganda kingship (or *Kabakaship*, the king being known as *Kabaka*), Ray provides magnificent views of symbolic and real interactions between the primal ancestor and the living *Kabaka*. The two orders reproduce other complementary orders about which he could have theorized a bit more: the myth or legends (*Lugero*) that illuminate the beginnings, and the historical narratives (*byafaayo*) witnessing to the foundation of the kingdom some time in the late thirteenth century. In what sense are the *byafaayo* different from the *Lugero* and what are the features that allow them to be qualified as historical?

The rupture from the mythical is interestingly incarnated by the same Kintu who actualized the genesis of a culture. The event is important. According to a number of students of central and southern Africa (Alexis Kagame, for example), *Kintu* (designation of a being without intelligence, or in fact a "being-in-itself") is part of a table of basic linguistic categories that would include *Muntu*, or being of intelligence; *Hantu*, which expresses "time" and "place"; and *Kuntu*, designating modalities. An examination of linguistic data from the south of Buganda and the area of the Great Lakes could have enriched Ray's description of the mutation incarnated by Kintu.

As symbol of the state, the *Kabaka* was treated as "most sacred"; hence the ambiguity of his body. Ray offers good descriptions of this ambiguity apropos codes of etiquette, the royal corpse, the royal mediums, and so forth. Luc de Heusch has observed and studied the same phenomena in the Bantu-speaking areas. By paying more careful attention to Heusch's *Écrits sur la Royauté Sacrée* (1987) and *The Drunken King* (1982), Ray could have clarified Frazer's notion of divine kingship and enriched his study of the Buganda Kabakaship.

The ensuing chapters deal with three themes: the royal shrines, regicide and ritual homicide, and Buganda and ancient Egypt. The first two chapters are interdependent. The royal shrines represent a political power whose inscription on earth is linked to the symbolism of the king's body. In the last chapter, Ray cautiously analyzes "speculation and evidence" about a connection between Buganda and ancient Egypt. Focusing on E. A. Wallis Budge's arguments concerning the African origin of Egyptian religion and Charles G. Seligman's racial theory, Ray notes "that important similarities do exist between Buganda and ancient Egypt, some of which are quite striking" (p. 197) and prudently adds: "the problem with the comparative work of Budge, Seligman, and Frankfort is that it was so entangled with historical, anthropological, and interpretive issues that it was less than illuminating" (p. 199).

Ray's book includes an introduction, a conclusion on the symbolic centrality of the king, an appendix, and a bibliography. All in all, he has written a well-researched and carefully argued work on Buganda.

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Steven Feierman. *Peasant Intellectuals: Anthropology and History in Tanzania*. Madison: University of Wisconsin Press. 1990. Pp. xii, 340. Cloth $49.50, paper $19.95.

Isaria N. Kimambo. *Penetration and Protest in Tanzania: The Impact of the World Economy on the Pare 1860–1960*. (Eastern African Studies.) Athens: Ohio University Press or James Currey, London, or Tanzania Publishing House, Dar es Salaam, or Heinemann, Kenya, Nairobi. 1991. Pp. xii, 188. Cloth $29.95, paper $16.95.

Tanzania first came to the forefront of African historiography in the 1960s as nationalist historians sought to establish African roles in history for the first time. Tanzanian historians then returned to imperialism in the 1970s to explore the roots of African underdevelopment and continuing dependency on the West. Now a third wave of Tanzanian scholarship is emerging that seeks to mediate the false dichotomy between local and global perspectives posited earlier. Such studies seek to regain the focus of the nationalist school on the particular histories of Tanzania and Tanzanians while placing these histories within the wider context of colonial capitalism.

Interestingly, both books reviewed here are deliberate reinterpretations of research first conducted in the 1960s. (See Steven Feierman, *The Shambaa Kingdom: A History* [1974] and Isaria N. Kimambo, *A Political History of the Pare of Tanzania, c. 1500–1900* [1969]. Another recent reinterpretation of earlier work along similar lines is Abdul Sheriff, *Slaves, Spices and Ivory in Zanzibar: Integration of an East African Commercial Empire into the World Economy, 1770–1973* [1987].) Both authors also have similar stories to tell, but they tell them very differently. Isaria N. Kimambo moves from dependency theory to explore the articulation of modes of production during the penetration of capitalism and the establishment of colonial rule in Pare. Steven Feierman assumes much of this analysis and focuses on the history of a discourse among Shambaa peasant leaders—the "intellectuals" of the title—concerning rain and the relationship between legitimate authority and social well-being in order to examine the ways peasants conceptualized, reflected on, and acted on these processes.

First, the stories, starting in the early nineteenth century. Pare and Shambaa occupied adjacent highland areas in northeastern Tanzania and pursued highly dependable and productive agricultural systems that supported dense populations and centralized political structures. Because the necessary productive and reproductive resources of land, labor,

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and cattle were all controlled locally, however, chiefs were dependent on their ability to extract tribute in return for providing valuable services. South Pare chiefdoms were small localized lineage aggregations, and chiefs gained tribute by controlling rain medicine and mediating disputes among lineages. In north Pare, by contrast, a centralized state had emerged in which the king also controlled initiation rites, distributed new land, and appointed his relatives to head subsidiary chiefdoms, while a similar centralized kingdom in Shambaa had developed around the king's control over rain and his subordinates. In both cases, cattle, food, women, and communal labor flowed to the king as tribute, while kings brought rain, insured social prosperity, and provided refuge for the weak.

The development of caravan routes skirting the highlands during the nineteenth century had profound effects on both peoples, as market centers were established at the base of the mountains to supply food, ivory, and, later, slaves to the coastal caravans in exchange for cloth, beads, and guns. As the loci of resources and power shifted from the highlands to their peripheries, aspiring chiefs moved to the plains and formed armed bands to raid for ivory and slaves. The resulting chaos left the Shambaa kingdom in ruins, new chieftaincies proliferated in Pare, and chiefs abandoned their roles as social guardians to raid their own peoples.

The German conquest in the late 1890s replaced slave raiding with its own bloody "pacification" campaigns and forced labor. And in choosing literate, Muslim, coastal traders as their local agents, the Germans further introduced regional trade and values—taxes, roads, shops, new crops, plantations, Islam, Christianity, and education—into the heart of the highlands, propelling peasants into migrant labor and crop production to earn money (Feierman). World War I and the heavy demands made by German and British troops for porters, food, and labor put additional strains on highlands' economies, resulting in the first great colonial famine (Kimambo).

The British rulers who took over Tanzania after the war opted to reverse German regionalization in favor of coopting local chiefs in a successful attempt to localize, and so contain, African politics and political discourse (Feierman). Parallel economic policies inconsistently stressed self-sufficiency and cash-crop production: government officers in Pare uprooted established coffee trees and disrupted marketing to promote rice and cotton, while they encouraged coffee growing over tea in Shambaa to protect local settlers.

As the pressures on land and labor increasingly threatened peasant subsistence in the 1940s, however, incipient crises resulted. Feierman details how local associations of Shambaa peasants protested that the British had appointed a king who lacked rain medicines, thus causing the agricultural crisis, and forced his resignation. The British saw things differently, attributing the crisis to poor land management rather than shortages. They appointed another king without rain and initiated a massive campaign to force peasants to abandon cultivated land deemed at risk and to consume endless hours of additional labor digging tie-ridges (*matuta*). Shambaa resisted in any way they could—refusing work, sabotaging field markers, boycotting settlers, demonstrating openly, allying with nationalists—and after a ten-year war of attrition, the British finally gave up.

At the same time, according to Kimambo, popular demands for more schools in Pare led the government to propose a graduated tax (*mbiru*) to be assessed by the chiefs. Pare responded that such assessments could not be trusted, led to conflict, and hindered progress, and offered to pay an increased flat-rate tax instead. The government persisted, and Pare responded with nonviolent demonstrations lasting months at a time. They appealed over the heads of their discredited chiefs and local administration to the government and the United Nations, and women besieged local chiefs in their offices. The government eventually relented and instituted a mobilization campaign stressing community development, improved husbandry, literacy, schools, and roads that was enthusiastically supported by Pare. When it subsequently introduced a centrally directed conservation plan similar to that in Shambaa, however, support vanished, and the campaign had to be terminated.

The two protest movements had much in common. Both resulted from extreme pressures placed on land, labor, and, ultimately, peasant subsistence by capitalist penetration and colonial policies. Both specifically protested "foreign" practices: *mbiru* was a "foreign" tax suitable only for non-Africans (Kimambo, p. 97), while *matuta* fostered "a foreign style of farming" (Feierman, p. 188). Both were organized and led by peasants, not by chiefs or educated teachers and clerks. And both featured dramatic demonstrations by women, those most adversely affected by the diversion of land and male labor from subsistence to cash crops and migrant labor. Neither movement rejected "progress," however. Their leaders were often educated peasants and traders active in the cash economy. People generally embraced cash-crop agriculture and respected successful farmers and traders. And they began to entertain new ideas such as nationalism, democracy, and government by the educated.

The different ways Feierman and Kimambo have of telling their stories can be seen most clearly in how they explain the apparent contradictions between rejecting certain changes as "foreign" while embracing other changes. For Kimambo, the protest over *mbiru* signaled the failure of capitalism without capital, as peoples' attempts to open new land, increase productivity, and produce crops for the market were repeatedly sabotaged by lack of government support or investment. They supported changes that poten-

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tially benefited all Pare, and opposed those that exploited them and left them weaker than before.

For Feierman, the central issues related to peasant discourse about rain. Shambaa supported chiefs with rain medicines and opposed those without them. Chiefs without rain sold land to settlers instead of demanding the return of alienated land. They forced people to labor on *matuta*, threatening peasant subsistence by converting annual fields used by women and the landless to improved men's fields. The discourse about rain was thus also a discourse about the proper relationship between legitimately constituted authority and general social welfare, such that even wealthy peasants joined the movement to defend the moral right of all Shambaa to subsistence.

These interpretive differences result from the different analytical frameworks used by the two authors. Kimambo takes his from political economy and focuses on the contradictions within colonial capitalism that restricted its ability to transform peasant production. Feierman would agree, but he seeks further to understand the ways that peasants themselves conceptualized the process, affected it, and transformed themselves. For Kimambo, Pare struggled with the state to control access to resources; for Feierman, Shambaa also struggled among themselves to control and shape their discourse over the nature of authority and well-being.

Kimambo thus firmly locates dependency in local struggles between peasants and the state, while Feierman seeks to forge new conceptual ground to explore how peasants were able to command the multiple discourses available to them to effect structural change. Starting from Ferdinand de Saussure's distinction between *langue* ("language," a system) and *parole* ("speech," an individual act), Feierman focuses on the role of the competent speaker able to create distinctive speech acts only by following regular grammatical rules. From Anthony Giddens's and Pierre Bourdieu's parallel social formulations of "rule-governed creativity" and *habitus* (generation of "regulated improvisations"), he shows how peasant "intellectuals," in Antonio Gramsci's sense, acted as historical agents to reformulate and transform local and colonial cultural constructs to effect structural change. In so doing, Feierman has issued an exciting challenge to reformulate the ways we construct peasant history generally.

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PATRICK J. FURLONG. *Between Crown and Swastika: The Impact of the Radical Right on the Afrikaner Nationalist Movement in the Fascist Era*. Hanover, N.H.: Wesleyan University Press and University Press of New England. 1991. Pp. xxi, 344. $45.00.

Patrick J. Furlong's book deals with some of the most delicate issues in modern South African history. To what extent was Daniel Malan's National Party a "National Socialist" or "Fascist" organization? Is the present South African government a Nazi regime? Is apartheid merely a latter-day reincarnation of Nazi racial policy?

His book is not the first to ask these questions. Henry Gibbs's *Twilight in South Africa* (1950) was a forceful, albeit lurid, account of the allegedly fascist nature of the Malan regime. Brian Bunting's *Rise of the South African Reich* (1964) had its thesis in the title. Although of interest, neither is a work of serious scholarship. On the other side of the question, National Party supporters, often using a selective memory, prefer to see the 1930s and 1940s as a time of "superficial experiments with National Socialism" that were quickly abandoned in favor of a fundamental commitment to democracy (p. 207). Furlong's work, one of the few scholarly attempts to analyze the National Socialist roots of contemporary South Africa, takes a middle path between these points of view.

The book deals briefly with the activities of the German Nazi Party in South Africa, but concentrates on the links between the National Party and groups on the South African radical Right, such as the anti-Semitic Greyshirts of the 1930s and the paramilitary *Ossewabrandwag* (OB) of the war years. Furlong's goal is to see how the Nationalists were transformed by contact with *völkisch*, anti-Semitic, and anti-democratic ideology. Noting that many radical Rightists imprisoned during the war for subversive activities came to serve in the postwar Nationalist government, Furlong finds that the party of 1948 (the year of Malan's election victory) was very different from that of 1934 (when Malan split off from J. B. M. Hertzog to form the so-called Purified Nationalists). Explicitly National Socialist trends like a pseudo-scientific racism, a glorification of the "Aryan" qualities of the Afrikaner *volk*, and a call for racial apartheid became part of the Nationalist credo during the late 1930s and early 1940s, although Malan rejected the radical Right's call for an end to parliamentary democracy. For example, the OB intellectual Geoffrey Cronjé, a leading proponent of racialist ideology who condemned miscegenation and roused fears of the disappearance of the white race, found a home in the National Party after the war. So did Louis Weichardt, former leader of the Greyshirts who had once publicly described the Jews as "vermin." Malan was able to win radical Right voters in 1948 without making any serious electoral concessions to them precisely because his party had long before "taken the path of gradual, almost imperceptible, but nevertheless fundamental accommodation to its rivals on the Right" (p. 238). But while Malan's "new order" was certainly an authoritarian state, it never approached the level of Hitler's Germany or Stalin's Russia—or even, Furlong notes, one of the lesser dictatorships like Antonio de Oliveira Salazar's Portugal.

Furlong has produced a most impressive book: well written, exhaustively documented, and packed with

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insight into South African affairs, past and present. The question must inevitably arise, however, as to whether events have already passed up part of his analysis. Discussing the situation in the 1980s, he writes that, despite reforms in the apartheid system, National Party control was not a negotiable issue. "Power, once achieved, would not readily be surrendered" (p. 257). Negotiations between the F. W. De Klerk regime and black political groups, which are leading South Africa to a new era, may prove that statement wrong. But as Furlong points out, the National Party has weathered severe storms before. Perhaps it would not be surprising to see some form of National Party dominance retained even in a post-apartheid South Africa.

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ASIA

ANATOLY I. MARTYNOV. *The Ancient Art of Northern Asia*. Translated and edited by DEMITRI B. SHIMKIN and EDITH M. SHIMKIN. Urbana: University of Illinois Press. 1991. Pp. xvii, 300. $39.95.

This synthesis of the art history of ancient northern Asia, the vast lands from the Ob' to the Amur River watersheds, will be useful to both students and specialists. In six succinct chapters smoothly translated by Demitri B. Shimkin and Edith M. Shimkin, Anatoly I. Martynov reviews a vast quantity of material, much of it originally published in Russian. Many large illustrations complete the stylistic and iconographic analyses provided in the text.

A brief chapter on the scattered, puzzling art objects of the Paleolithic Age is followed by a review of the decorated pottery, sculpture, and petroglyphs of the Neolithic period. Describing regional styles in production, Martynov also interprets the function and meaning of the ancient imagery on the basis of comparative analogies and the mythology of northern peoples or other cultures in Europe, Asia, and the Near East. This framework is also adopted for the chapters on the pottery ornament and "Okunevo" statuary of the Bronze Age, the rich sculptural, metalwork, textile, and other traditions of "Scytho-Siberian" peoples (500–200 B.C.), and finally the diverse remains of the period of migrations and nomadic feudal states, including adornments, petroglyphs, burial masks, bronze castings, and stone statuary. The last chapter traces a single motif, the so-called "Tree of Life," throughout this history; here, although suggestive, Martynov's sweep is too broad.

Throughout, Martynov stresses the development of indigenous traditions in their own contexts, an approach Western art historians will favor. Thus, for example, he resists the view that the modern arts of Amur peoples diffused from Chinese sources; rather, he urges, the local traditions of the Amur artists can be traced to the Neolithic culture of the region (p. 18). Again, although he cites relations between Siberian "animal style" arts and those of the Black Sea or Near East, he concentrates on the specific nature of the style in northern Asia (pp. 52–73). Martynov sometimes proposes more global parallels for a technique or motif. In some cases, the analogies are justified. For example, small sculptures of bear and moose from the Neolithic Siberian forest zone should probably be related to similar works from northwestern Russia and Scandinavia (p. 21). But analogies with pharaonic Egypt, cited several times, are less revealing. Egypt's economic and political structures were quite different from those of ancient northern Asia, and Martynov's comparisons merely blur his focus.

Like most Soviet archaeologists, Martynov understands art to be determined by the particular ecological, technological, and economic circumstances of its actual producers. The supposed psychic unity of mankind and the universal trajectory of human social development imply, however, that commonalities among far-flung cultures might be recognized. Unfortunately for readers unable to consult Russian scholarship for themselves, Martynov provides little information about other aspects of the material culture of northern Asian traditions or about the archaeology of settlement, trade, mortuary practice, and the like, often failing to describe other features of the excavated sites from which some art objects derive. What archaeological evidence supports the idea, for example, that rituals were carried out on rock ledges below petroglyphs (p. 42)? When art is considered in isolation, it is difficult to evaluate its interpretation in terms of traditions or analogies. Despite this weakness, Martynov's excellent survey of many modes of ornament and imagery does serve as an essential reference on art-historical questions. The editors should be thanked for their careful work in making the book available.

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MARK EDWARD LEWIS. *Sanctioned Violence in Early China*. (SUNY Series in Chinese Philosophy and Culture.) Albany: State University of New York Press. 1990. Pp. viii, 374. $21.95.

No state can long remain neutral on the issue of sanctioned violence. But the precise ways in which the relationship between the state and violence might be conceptualized are by no means self-evident. Mark Edward Lewis discusses the major transformations in the relationship of the state to sanctioned violence in a particular time and place: China during the late Warring States period (fifth century to 221 B.C.). (The rubric "sanctioned violence" encompasses sacrifice, vendetta, and the hunt as well as warfare.) Lewis argues that changes in the way violence was imagined

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and mythologized are key to understanding the profound social, economic, political, and military changes that occurred during the Warring States period. He identifies the core of the late Warring States transformation as the "development of the unquestioned supremacy of a single, cosmically potent autocrat who ruled as the image of Heaven on earth." This development was accompanied by a reconstitution of the public order around the ruler (p. 246).

The book casts a net considerably wider than the title might imply. The Warring States period was the great era of classical Chinese philosophy, and violence and its relation to both nature and culture was an issue that profoundly engaged philosophers and mythologizers alike. The nature of political power (and its relation to sanctioned violence) was one of the questions that animated philosophical discourse. Lewis's book places ideas about violence deeply in their context, and as such illuminates both the ideas and their context.

In the eyes of thinkers of the Warring States period, violence occupied a paradoxical and problematic position. Central to Warring States social mythology are a series of culture heroes who taught human beings the technical skills (such as agriculture and writing) that distinguished the human world from the world of beasts. Violence was recognized as part of that world of beasts. Yet the socialization of violence, that is to say, its sanctioning, was central to the process of civilization. Lewis suggests that the Warring States argument that the very distinctions which separate human from beast were made by sage rulers is "perhaps the most radical claim to political authority that has ever been made" (p. 212). Thus, in this mythology, humankind itself exists as the result of an act of political will.

Lewis shows how changes in myths of violence were accompanied by changes in metaphors of gender. Warfare during the earlier Spring and Autumn period had been the privilege of the aristocracy, and authority sprang from it. But as the nature of warfare changed, and more and more men participated, the sources of authority changed. In what Lewis describes as a "sexual re-imagining" of politics, which happened during the Warring States period, "the male collegiality of shared combat was replaced by the domination of husband over wife as the model of authority" (p. 95).

This is a densely argued book, drawing on a variety of sources, archaeological as well as textual. The argument is bold: that violence and its theorization form a coherent way of looking at the Warring States transition. It is a contribution not only to the specific history of the Warring States period but also to the general problem of how to think about the role of violence in Chinese society. This book suggests ways in which violence and warfare were crucial in the imagining of the early Chinese state. An examination of the role of violence and ways in which it is imagined and re-imagined in later periods of Chinese history might cause us to refine our reading of the relationship between civil and martial values. This book would repay the careful attention of those interested in the role of violence and social imagining of violence in the process of state-building in other societies.

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LOREN BRANDT. *Commercialization and Agricultural Development: Central and Eastern China, 1870–1937*. New York: Cambridge University Press. 1989. Pp. xiii, 232. $42.50.

In this volume, Loren Brandt seeks to debunk the conventional view that commercialization aggravated agrarian poverty in China as manufactured imports displaced rural handicrafts and peasants became more vulnerable economically because of greater exposure to market volatility and shifting terms of trade against agriculture. Brandt argues that, first, commercialization of Chinese agriculture was driven not only by rising external demand for exports such as tea and silk but also by growing international influence on domestic prices and by the nonagricultural sector's expanding demand for food grains and cash crops. Second, in response to new demand and rising agricultural prices, peasants switched from home nonagricultural production to cash crops and worked the land more intensively. Brandt estimates that agricultural output grew at 1.2 to 1.5 percent per year, a rate about double that of population growth, and that rural incomes rose. Third, benefits from commercialization were evenly shared, as the influence land distribution had on rural income distribution was reduced due to rising real wages, the equal participation of farms of all sizes in new economic opportunities, and the growth of nonagricultural sidelines.

Brandt uses econometric methods to derive valuable new estimates. He convincingly shows that through international commodity arbitrage, prices in Shanghai and interior Yangtze markets were tied to overseas prices: the international price of rice and the gold price of silver (the basis of China's currency) accounted for 80 to 90 percent of the variability of Shanghai rice prices. Brandt's price indices demonstrate a secular rise in agricultural prices from the 1890s to the 1930s due to the depreciation of silver and the general rise in international agricultural prices. He further establishes a strong case for rising rural productivity and incomes by computing time series data showing rising agricultural wages, increased per capita consumption of cloth, and movement of the terms of trade in agriculture's favor.

Brandt's view of the Chinese economic system as an efficient market, however, exaggerates the degree of

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market integration and ignores qualitative evidence of market imperfections and constraints on factor markets. The multiple layers of brokers and middlemen substantially depressed prices received by peasant sellers of agricultural products and raised prices paid by peasant buyers of manufactured goods relative to Shanghai levels. Moreover, the large number of buyers and sellers did not necessarily mean a competitive market. In the rice trade, commercial firms established exclusive contracts with particular peasant households to which they had advanced credit or fertilizers. Despite some improvements in infrastructure, rural production and marketing remained hampered by the decline of irrigation systems, the lack of credit, and transportation bottlenecks.

Brandt's econometric approach downplays the impact of political factors and population pressure and is limited by the quality of available data, which becomes progressively poorer and scarcer as one moves down marketing levels from the treaty ports to rural markets to peasant households. While Brandt has established that aggregate rural production and incomes rose, he has not addressed the vulnerability of peasants to market volatilities. Yet seasonal fluctuations, which could be as much as 40 to 60 percent, and short-term disruptions due to such political factors as irregular taxation, military campaigns, and banditry could cause financial ruin. Evidence for Brandt's proposition that peasants in Central and East China were profit maximizers who participated equally in market opportunities regardless of economic status is derived from North China data, a highly questionable procedure given important regional differences in the ecological environment and the degree of commercialization. His thesis that agricultural commercialization equalized income distribution must be tempered by the unexplored possibility that landlords might have benefited disproportionately from new commercial opportunities through usury and direct involvement in trade and industry.

Brandt's methodologically sophisticated interpretation therefore is an important addition to the debates on Chinese agrarian history and shows the power as well as the limitations of the econometric approach.

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Kyoko Inoue. *MacArthur's Japanese Constitution: A Linguistic and Cultural Study of Its Making*. Chicago: University of Chicago Press. 1991. Pp. xii, 378. $39.95.

This valuable study of the postwar Japanese constitution has been written by a linguist, but it is of considerable interest to historians. Although the bulk of Kyoko Inoue's discussion deals with nuances and subtleties in the Japanese translation of the American draft prepared by General Douglas MacArthur's staff and will thus be inaccessible to those without a substantial knowledge of the language, at least they can appreciate what she is trying to do and learn from many of her insightful observations.

The book offers a thorough comparison of the original American draft, the constitution as it was finally composed and adopted, and its Japanese translation. Before the first became transformed into the second, much negotiation took place between American and Japanese officials, with the latter trying to modify the language and substance of such matters as the status of the emperor, "rights and duties" of citizens, and the separation of church and state. Even more interesting, between the version the occupation authorities and Japanese officials agreed on and the Japanese translation of it, there were some significant disparities because the translated version sought to put English words and phrases into not necessarily literally equivalent Japanese but into terms more comprehensible in the context of Japan's political culture.

This is a fascinating project, and one wonders why no historian has attempted the task. Inoue makes some assertions that may not be acceptable to historians. For instance, they may question her categorical statements that "the Japanese Emperor never acted alone, and had no real power under the Meiji Constitution," that he "never actively participated in [cabinet] discussions" but "sat silently while the Cabinet members deliberated among themselves," or that "most Japanese wanted to retain the imperial institution" after the war "and continue to revere their Emperor" (pp. 167, 182, 187). The emperor never attended cabinet sessions, but presided over and actively spoke at the meetings of high civilian and army officials.

These criticisms, however, pale beside the fascinating analysis the book offers of the ways in which the American draft turned into the Japanese constitution. For example, regarding the emperor's status, article 1 of the American draft had stated, "The Emperor shall be the symbol of the State and of the Unity of the People, deriving his position from the sovereign will of the People, and from no other source." This was changed in the final version to: "The Emperor shall be the symbol of the State and of the unity of the people, deriving his position from the will of the people with whom resides sovereign power." A seemingly innocuous change, it was a product of much debate and negotiation. At first the Japanese government translated "sovereign" into a Japanese word that suggested "supreme will" in an attempt, the author says, "to render the idea of popular sovereignty obscure" (p. 206). The Americans objected, suspicious "that Japanese government officials were trying to retain as much of their existing political system as possible" (p. 210). When the Americans refused to budge, the Japanese agreed to render "sovereign" into "shuken," which is the proper translation, but then rephrased the article to make sure

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that, rather than seeing the emperor as a creation of the people, he would now be considered one of them, possessing sovereignty just as they did.

There are many other examples of this kind that show that the Japanese constitution, far from being imposed by American occupation authorities, was in fact a document with much input by Japanese officials. They tried and often succeeded in changing words and phrases, and even when they failed, they altered the meaning of some key concepts in the process of rendering them into Japanese.

This is an important study and should be read not just by specialists in Japanese studies but also by those who are interested in cultural dissemination and transformation.

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ANDRÉ WINK. *Al-Hind: The Making of the Indo-Islamic World.* Volume 1, *Early Medieval India and the Expansion of Islam, 7th–11th Centuries.* New York: E. J. Brill. 1990. Pp. viii, 396.

André Wink stands prominent among scholars currently writing about the history of the Indian Ocean region during the pre-European period. In his projected five-volume series, Wink proposes to analyze the region's economic and political changes, concentrating on India, from the seventh through the eighteenth centuries. This first volume establishes the theoretical and geographical context for the series and then concentrates on the initial Islamic-led expansions and influences (seventh to eleventh centuries).

Recently, several excellent studies have argued that the region stretching from China and Southeast Asia to West Asia and East Africa—in many ways centered on the Indian subcontinent—had already been economically unified prior to the European arrival. European penetration, however, came at a time of temporary fragmentation and was thus fundamentally a reconstruction of preexisting segments. Among those who contribute to this argument are K. N. Chaudhuri (*Trade and Civilisation in the Indian Ocean: An Economic History from the Rise of Islam to 1750* [1985]) and Janet Abu-Lughod (*Before European Hegemony: The World System A.D. 1250–1350* [1989]). Wink's project promises to be the most extensive of these efforts.

Wink holds that the Islamic world system developed three main characteristics. First, Islam advanced the economy's monetization. Muslim invasions established a uniform bimetal currency and circulated the vast bullion hitherto concentrated in India's temples and royal treasuries. Officials of these Muslim states held *iqṭā's* (transferable assignments of land revenues) rather than personal fiefs. Much of the ensuing trade expansion came through "trade diaspora" communities, primarily Jews, Muslims, and Parsis already scattered on the Indian Ocean littoral. Second, reliance on slave administrators, often converts to Islam, allowed a more expansive political apparatus. Although these officials had no intrinsic regional roots in the areas they administered, they nevertheless on occasion split off through *fitna* (calculated sedition) to form new polities (p. 13). Third, the initial Arab advance merged quickly into Persian administrative models. Islam recentered on the Abbasid Caliphate (from 749) in Baghdad. Subsequent Islamic expansions came under Persianized rulers and officials, often Turks from central Asia.

Wink's book masterfully synthesizes Arabic and Persian texts, along with much secondary literature on this period, into a systematic picture of how Al-Hind (the eastern frontier of the Islamic world) appeared to Muslims. Chapter 4 provides capsule histories of the political geography of these regions and their conquest by Muslims. The final chapter surveys the unconquered regions of India and Southeast Asia, asserting that the Islamic expansion shaped the political and economic developments even in these states. These studies present a fine overview of a period that had been conventionally dismissed as disordered and feudal.

Wink's conceptual synthesis of this period and these sources is a major contribution to scholarship on the eastward expansion of Islam. Historians of the Islamic world, precolonial South Asia, or European and non-European world systems will find Wink's first volume a valuable beginning to what promises to be a significant series. Those wishing to avoid the $82.50 price charged by Brill can obtain the Oxford University Press (Delhi) edition (Rupees 250) through South Asian book importers.

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K. N. CHAUDHURI. *Asia before Europe: Economy and Civilisation of the Indian Ocean from the Rise of Islam to 1750.* New York: Cambridge University Press. 1990. Pp. xviii, 477. Cloth $69.50, paper $19.95.

In an *AHR* forum article, Ian Tyrell suggested that comparative history had certain inherent defects, the most obvious of which was the tendency to compare whole countries and to take for granted the primacy of a national unit of analysis, with a stress on national differences ("American Exceptionalism in an Age of International History," *AHR* 96, [1991], 1035). However relevant this criticism may be to most exercises in comparative history, it is certainly not for K. N. Chaudhuri's complex study of the regions around the Indian Ocean from the rise of Islam in the seventh century to the middle of the eighteenth century. National units as such have little place in the work, nor is much attention paid to chronology. In reconstructing the history of the vast area, his concern is

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with the interplay of society, science, technology, and daily life: the *longue durée* of Fernand Braudel.

Chaudhuri gives fair warning to the reader by noting that he uses axioms of set theory and other mathematical constructs that may seem difficult to anyone accustomed to thinking in terms of historical sequence. With this in mind, Chaudhuri provides a glossary of his technical terms, although this does not in fact help lessen some of the book's obscurities. Despite this, however, the book raises issues and provides insights that will enrich the thinking of any serious student of the nature of the historical process in a region and in a period vital to both Asia and Europe.

Throughout the work Chaudhuri tries to integrate three features in a unified study of the Indian Ocean littoral. One is to both construct a theory of comparative history and to use it as the basis for his analysis; another is to identify stable social and economic structures in the region; the third, and perhaps the most difficult given their complexity, is to chart the nature of the historical processes. He begins with an examination of the "unities of discourse," under which rubric he includes both spatial and temporal connections. Throughout the study he comments on the civilizations of the Chinese, Indians, Cambodian-Javanese, and Arabs (a term that includes other Islamic peoples), and attempts to show that each of these civilizations has its own identity in terms of its particular historical experience, modes of production and distribution, and the role of the state, but that each of these items can be used across the cultural frontiers for a generalized comparative discourse. Not all readers will agree with his sketches of these civilizations, especially assertions such as that "no Arab or Persian thinkers ever approached the intellectual depth of a Sankara or a Ramanujam" (p. 53). When he draws up a balance sheet of the great civilizations he is, however, even-handed in indicating their failures.

Turning from theoretical considerations, which comprise over a third of the book, Chaudhuri provides case studies of how people throughout the region housed themselves, what they ate, and what they wore. It was in terms of the answers that travelers gave to these three questions that they recognized other civilizations. This emphasis, Chaudhuri insists, was not superficial, for all the sources for comparative history, pictures as well as written accounts, confirm that these fundamental material considerations of food, clothing, and housing were, for outside observers, the symbols of social identities derived from religious and moral systems. Although he does not cite them, support for his argument can be found in the almost obsessive interest shown in the nineteenth century, especially in India, by foreigners in sketching and photographing the clothing, handicrafts, and houses of the people. The meticulous ethnographic work of modern anthropologists points in the same direction, although they may deny that the material culture is grounded in religious ideologies. When travelers from one cultural tradition met with people from another, they had the option of changing their identities, at least for a short time, by trying the other's food. As a seventeenth-century French traveler in Persia and India noted, "Religions divide people in their Food, as well in their Beliefs and Worships" (p. 156). Clothing was an even greater measure of identity, Chaudhuri argues, with people being less willing to change their clothing habits than their food.

Chaudhuri pursues his theories through an astonishing array of material artifacts, and he is especially interesting on the symbolic role of architecture, both for public and private purposes. Agriculture is given considerable attention, including the irrigation systems of the Indian Ocean region, and he suggests that the theory of "Oriental Despotism" based on water control disregards the finer details of crop production. Chaudhuri also relates urbanization in the region to his general theories, and his distinction between the "perennial" city, such as Nanking in China, and the "ephemeral" city, such as Fathpur Sikri in India, leads him to interesting generalizations.

At the end, he considers the work of Immanuel Wallerstein in relation to his own, and while agreeing with the general thrust of the world-systems argument, he would give more place in the creation of power relations, especially in the Islamic world, to political, cultural, religious, and linguistic factors. As Chaudhuri remarks in his preface, this book is just the beginning of the the task of reconstructing the historical past of the Indian Ocean; it is, however, an important milepost.

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Susan Bayly. *Saints, Goddesses, and Kings: Muslims and Christians in South Indian Society, 1700–1900.* (Cambridge South Asian Studies, number 43.) New York: Cambridge University Press. 1989. Pp. xv, 504. $59.50.

Susan Bayly's narrative of Muslims and Christians in South India covers an unusually long period and does double work in looking at two religions. Most of all, it is impressive in that it incorporates both the "folk" and the "classical" levels of religion, arguing that these divisions do not in fact operate very often. But for scholarship they do, in the sense that scholars tend typically to study either state-community relations or intracommunity processes. Bayly exhaustively—but not exhaustingly—does both.

The first part of the book deals with Muslims, centering on two topics: the spread of Islam in Tamilnad and the patronage of the Nawabi of Arcot. She makes a number of important points that reflect the ongoing debates on South Asian Islam. First, regarding syncretism, she is cautious in her pro-

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nouncements, but argues quite persuasively that "Instead of relating the devotee to the various stages of Muslim history and pre-history, the cultural traditions . . . have come to focus on motifs and episodes which invest the region's pirs with attributes of Hindu divine power" (pp. 115–16). She describes cults, their Saivite, Vaisnavite, or Goddess motifs, and their acceptance by worshippers.

A second significant finding relates to the lack of separation between sacred and political-military functions of shrines. Among many confirmations offered of this is the description of circumambulation of the shrines by the nawab: "By making contact with such sites, Azam Jah and his retainers were tracing out and reoccupying the great grid of supernatural pir domains—the vilayats—on which the nawab's predecessors had originally built their rule" (p. 233).

One question that arises is, why was there not greater "Islamisation" of the area? And why did Islam become indigenized, rather than, as in other parts of the Muslim world, received as a universal, transcendent force? Bayly discounts material conditions as being only partially explanatory and gives importance to the nature of indigenous political structure, society, and religious traditions. This explanation works better for politics and society than for religion, perhaps because religion is more difficult to document. Religion is seen almost as a legitimation of political and commercial needs, the acceptance of Hinduism by the smaller southern kingdoms as "largely a matter of historical timing" (p. 237).

In the second part of the book, Bayly discusses Christians in four sections: the St. Thomas or Syrian Christians of Kerala, the Paravas of coastal Tamilnad, and two Christian traditions of the Tamil hinterland. The discussion reveals a striking similarity between Christian and Muslim history in South India. If the narrative were not so well documented, one might suspect the author of a bias in this regard. There is the comparison of Christian tomb shrines with Muslim pir cults, and of the Muslim urs ceremony with the Christian death anniversary of the prelate-saint. There are further comparisons: both religions recite histories of voyages and ordeals, both cite martial leaders and triumphs. Remembering that Islam in South India has already been compared with Hinduism, what do all these comparisons imply? That given the structural and symbolic parallelism, Hindus, Muslims, and Christians may be expected to be well integrated? That the influence of the dominant religion, Hinduism, is all-pervasive? That political and structural stability is really the issue, and which religion is "chosen" largely a matter of historical timing? Or, perhaps, that symbols in human repertoire are rich but limited?

Bayly herself does not ask these questions, and of course it is easier to pose them than to try to answer them. In her conclusion she reiterates some of her earlier findings, such as the reciprocity of the political and religious domains, and the necessity of relating shrines, divinities, and cults to regimes and ruling lineages. Furthermore, folk-high divisions are untenable because in the eighteenth and nineteenth centuries there was neither a phenomenon called Hinduism nor, for that matter, caste as we know it, only local traditions. Although she does not, or cannot, explain why so much indigenization and assimilation took place (only to be followed by so much communal tension), there is an inclination toward the religion-as-political legitimation argument. Indeed, she has enough data to support it, but she does not make any overt claims at discovery. Her book is rich with documentation and careful narration, but her style is modest (unlike that of many historians of South India), and so are her claims.

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PENDEREL MOON. *The British Conquest and Dominion of India*. London: Duckworth; distributed by Indiana University Press, Bloomington. 1989. Pp. x, 1235. $89.95.

Penderel Moon provides a fitting characterization of his own book when he terms it "an old-fashioned, conventional history, concentrating on the deeds, motives and thoughts of the British actors in the drama of events rather than on the almost unmoving background of the mass of population against which they played their parts" (p. 9). Indeed, it follows in a long line of historical writings by administrator-historians (the author was a member of the Indian Civil Service) who used India as a backdrop to illuminate the lives of great British men. Even the central question it sets out to answer—"How was it possible for a mere handful of foreigners from a distant island to make themselves masters of a large sub-continent, the inhabitants of which numbered many millions?" (p. 4)—has a Macaulayesque ring to it. So does the spirit that animates the account. As Moon asserts, "Whatever moral judgements may be passed on this empire—and it has been both lauded and execrated with undiscriminating fervour—its establishment was an achievement that certainly ought to excite wonder" (p. 4). In one respect, however, Moon differs from Macaulay: his "wonder" is not reserved solely for men such as Robert Clive and Warren Hastings but is also extended to men like himself who toiled in the shadows of the governors general and the viceroys.

Beginning in the mid-eighteenth century, the first two-thirds of this massive tome takes up the story of the initial century of colonial rule and closes with the rebellion of 1857–58 (referred to as the "Mutiny" in this account), after which the crown assumed direct and complete control of the government of India. Part 2, occupying the remaining third of the book, carries the chronicle down to 1947, the year of

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independence and partition of the subcontinent into India and Pakistan.

Moon lavishes attention on the first century because he claims that the latter period lacked "exciting and dramatic episodes, and the historical figures, on the British side at least, not having the same scope for individual initiative, are less arresting than the heroes of the Company's days" (p. 761). His "heroes" range from the English officers on the battlefield whose Indian sepoy armies established British military supremacy to the governors general (for example, Hastings, Lord Cornwallis, Marquess Wellesley, William Bentinck) whose terms in office were apparently instrumental in consolidating and developing the emerging colonial empire.

In part 2 of the book, viceroys and lesser administrative mortals share the limelight with Indian personalities, Mahatma Gandhi and Mohammed Ali Jinnah particularly receiving the lion's share of the attention. Much of this section is written in the vein that British rule had outlasted its purpose and function by 1947, when "it could not hope to grapple successfully with India's pressing social and economic problems or to satisfy the aspirations of educated Indians. If the British had been bold enough and, uninfluenced by the glamour of empire, wise enough to launch India as a dominion some fifteen to twenty years earlier, much bitterness and feelings of frustration, perhaps even the tragedy of Partition, would have been avoided" (p. 1187).

Few historians today are likely to pen this kind of "old-fashioned" account, and probably fewer still would do so after reading this comprehensive blow-by-blow account. Nor can scholars today justify pursuing this genre of political narrative at the expense of ignoring the riches of social, cultural, and economic history that are now available in the field of South Asian studies. Nor are apologies for the Raj much in vogue any more.

Anand A. Yang
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Gyanendra Pandey. *The Construction of Communalism in Colonial North India.* New York: Oxford University Press. 1990. Pp. xvi, 297. $28.00.

In this volume, Gyanendra Pandey explores the development of modern politicized ethnic or communal loyalties in the context of British imperial rule. Analysts of this problem have long worked in well-worn grooves. Pandey, however, provides fresh and original insights, uses conventional materials in a new manner, and explores heretofore unknown sources. Pandey's study is motivated throughout by a moral urgency wholly understandable given the tragic expression of Hindu "fundamentalism" and ethnic violence in India today.

This important book should find many audiences, and not merely those interested in the history, politics, anthropology, and religions of modern India. One audience might be those concerned with the power of ethnic identities throughout the world today and perhaps most notably in Eastern Europe. Pandey clearly demonstrates that communalism in the subcontinent was a modern development that arose in tandem with, and out of the same context as, modern nationalism. The commonplace assumption that ethnicities today are "reemerging" after long suppression, as if they were primordial identities ever lurking underground, must be replaced by concern with precisely how those ethnicities have been constructed, often precisely by those governing powers that are presumed to have kept them in check.

A second audience, perhaps less predictable, should be those concerned with European traditions of historiography. Pandey shows us the invention of histories for India, the evolution of a "common sense" that selects and distorts the story of India's past in order to make communal essentialism the driving power in that history throughout. The "Whiggishness" of much of Indian historical writing is a bleak legacy of the culture of those who gave us the term. Why that "history" was so embraced by Indians themselves is not clear. But it is, today, harder to think of a society where justifications from history are more a part of public debate.

Pandey's chapter, "The Colonial Construction of the Indian Past," is particularly remarkable for showing the emergence of the central themes in the British historiography of India's past. Pandey employs the simple but compelling technique of examining British reports of a single disturbance, from the time of its occurrence in Banaras in 1809–10 until the report of it in the gazetteer of 1907. In that period recountings of the event became ever more fixed in what was increasingly taken as the only possible explanatory model: it was a communal riot, motivated by fanaticism, irrationally triggered by an unexpected dispute. Not only does Pandey show how the story, presumably unconsciously, comes to fit the "authorized" narrative of British rule but he also gives us both here and in other chapters a vivid sense of the world of change and dislocation, the wrenching transformation of economic and social and political life, that the convenient all-purpose explanation of religion effectively veiled.

Most of the material in the book has been published elsewhere in substantially the same form: this is not, but should have been, indicated clearly in the volume. It is nonetheless of great value to have the material presented as a single argument. Moreover, one hopes that scholars outside the South Asian field will begin to take note of a work such as this, clearly of significant comparative interest, now that it is available in readily accessible form.

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HENRY P. FREI. *Japan's Southward Advance and Australia: From the Sixteenth Century to World War II*. Honolulu: University of Hawaii Press. 1991. Pp. xvii, 303. $32.00.

In addition to containing some valuable insights, this book by Henry P. Frei provides convincing support for the long-held scholarly premise that, prior to the outbreak of World War II in the Pacific, Japan never had any intention of invading Australia. Indeed, the author states on the first page of his preface, "'Was Japan a threat to Australia?' is the basic question this book asks" (p. ix), and he consistently replies to his own inquiry strongly in the negative throughout his text. That hypothesis and specific examples to bolster it might have made a succinct article or even a useful monograph.

The book, however, is unfortunately exemplary of the apparent pressure on aspiring academics to publish seemingly unedited dissertations. Furthermore, this volume also seems to evidence the increasingly lax standards of university presses eager to fulfill their publishing quotas and, in turn, to justify their subsidized existences. The story to be told of Japan's relations with Australia up to the Pacific war is certainly an exciting and fascinating one. The pioneering detailed scholarship of D. C. S. Sissons (only some of which is briefly mentioned) and the current work of such scholars as Regina Ganter and Kyūhara Shūji (both apparently unknown to Frei) indicate well what a potentially rewarding enterprise research in this field could be.

Frei's book, however, is characterized by a deluge of superfluous verbiage. Moreover, his reiteration of well-known history for "background purposes" may be appropriate to a dissertation, but it certainly does not help to attract and to inform the scholarly reader. For example, until page 39 the word "Australia" does not even appear, and between pages 136 and 149 "Australia" is once more omitted. Frei also lists a plethora of named Japanese who are not even identified by dates of birth and death, let alone by any biographical information. At best we are given such lame data as "a prominent economist" (p. 46). Who were these individual Japanese who wrote about Australia in the late nineteenth and early twentieth century? Who published their work? More importantly, who read their essays, journals, diaries, or whatever? What, if anything, was the impact of these efforts?

Perhaps even more egregious for the academic reader was the decision of the University of Hawaii Press to use unnumbered endnotes in lieu of footnotes. Was this an economy measure of some kind? Whatever its provenance, it has certainly not served Frei well and is of little use to the concerned reader. If this is a new trend in historical publication, may I herewith publicly deplore it.

One of Frei's most informative findings is that in the early twentieth century Australians "in Japanese pay" (p. 105) were being employed to counteract anti-Japanese attitudes in Australia itself. Although Frei, in fact, gives only a single instance of this phenomenon, he might well have made much more of this and drawn stimulating analogies with similar documented practices of the same period in, for example, the Philippines and the United States.

Sadly, too, there is only a single reference to the Japanese prostitutes (*karayuki-san*) who were a significant component of the pre–World War I Japanese population of Australia and whose myriad economic activities paved the way for the subsequent significant entrepreneurial activities in Australia of the great trading firms like Mitsui, Mitsubishi, and others.

I believe that with careful supervision, conscientious editing, and better attention to significant sources, Frei ultimately has the ability to do important work in the history of modern Japanese-Australian relations. I for one sincerely hope that he will have that opportunity.

GRANT K. GOODMAN
EMERITUS
University of Kansas

JAN CRITCHETT. *A "Distant Field of Murder": Western District Frontiers, 1834–1848*. Carlton, Victoria: Melbourne University Press; distributed by International Specialized Book Services, Portland, Oreg. 1990. Pp. xiv, 303. $34.95.

On the margins of British expansion struggles ensued between possessors and dispossessed. This was as true in Canada as it was in New Zealand, in South Africa as it was in Australia. In all areas of influence natives responded to the new imperium. In some cases they were passive or less demonstrative of their grievances: in others, they responded with armed force. In one way or another they made known to the agents of British empire that their patrimony, their economy, and their society were being subverted.

The features of aboriginal responses to British rule have long been obscure and only recently been rescued by C. D. Rowley and especially Henry Reynolds. The resurgence of native political activity and the recognition by the government of Australia of aboriginal customary law have given impetus to aboriginal history and cross-cultural study. For these reasons alone the appearance of Jan Critchett's case study of frontier interactions in the western district of the colony of Victoria during the 1830s and 1840s is exceedingly welcome. The close study of frontier process is all the more valuable because we gain an enhanced appreciation of why troubled natives responded the way they did, why befuddled officials attempted to prevent wanton murders against white pastoralists, and how perplexed newcomers struggled to establish themselves. Murder seemed an everyday occurrence in this remote corner of the empire. The police, convict constables, were insolent, unreliable,

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overworked, and unsupported by the magistracy. Settlers demanded justice. One appealed to Lord John Russell, the Colonial Secretary, "surely it is time that England should interfere, and no longer permit those daily murders to be perpetuated with iniquity . . . I should indeed regret . . . to make it a matter of Parliamentary inquiry but . . . I owe it to the honour of England, whose colony this distant field of murder is" (pp. 114–15). The murdering was done by both sides, as Critchett so carefully demonstrates, and by the close of the fourth decade of rule the authority of empire was ensured. She calls the new area of authority "the convincing ground" and adds that here the emphasis was not so much on civilizing and Christianizing the aborigines as on making them realize that they had to accept the new regime. Frontier violence was a gratuitous aggravation of empire, as all colonial secretaries and their underlings knew. Now, for the first time, we can appreciate the native responses, both in quantity and time. This well-written, well-documented monograph possesses an extended bibliography, reliable index, and enticing photographs. Three appendices, listing aboriginal tribes and census details, whites killed by aborigines, and aborigines killed by whites, provide a rich texture and quantifiable evidence for the theme.

This significant contribution to Australian history and frontier studies could have been enriched by a deeper appreciation of imperial experiences on other frontiers and an in-depth understanding of London's responses to struggles in this particular frontier. Even so, the themes of this work are universal, and those who have dismissed British imperial history as somewhat redundant to today's scholarship had better think again. For what was being worked out on the margins of Australia's settlements was a key to why Australians suppressed the violent details of their past. As with the attempt to minimize convict settlement, the desire, conscious or otherwise, to minimize aboriginal resistance to European settlers is a denial of history. Thus, Critchett's work, even if on a limited scale, reveals vistas to much wider horizons of promise to her and to others.

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Stuart Macintyre. *The Oxford History of Australia*. Volume 4, *1901–1942: The Succeeding Age*. New York: Oxford University Press. 1986. Pp. xx, 399. $36.00.

What strategy should a general historian adopt in the age of historical specialization? Is it possible to combine the sophisticated methods and the history-from-below concerns of the new social history with general history's traditional, elitist concentration on high politics? More importantly, who is to judge if general history succeeds? If professional historians are talking only to their fellow subdisciplinary specialists how can we be certain that we know what a nonprofessional audience wants from its history books?

The traditional model of general history in fact confused generality with popularization. Proclaiming a new grand synthesis, general history in its heyday really only provided a more accessible and inclusive account of one approach, the story of "the public endeavours of influential men," as Stuart Macintyre says in his preface (p. x). More recently missionaries for newer approaches have also sought a wider audience, but in their attempt to put on demotic dress they have produced a kind of "Country Road" history, slick and popular in a commercial sense, but lacking the seriousness and authority of the general histories written in the imperial age when politics was intellectually reputable.

Macintyre aims in this book to break with the "high politics" of traditional general history but still to insist on the centrality of politics. He also wants to reconstruct "the full range of social relationships and the texture of every day life" (p. x). The bridge between politics and social life is power. Macintyre relies on a theory of social relationships as power relationships; in the attempt "to grasp these social relationships in their totality . . . we will not understand the popular experience unless we recognise the effects of structures of power . . . Politics, properly understood, is not an end in itself but a response to social relationships" (p. x). In these phrases we might detect traces of his earlier interest in Marxist class analysis, an interest now enlarged by the impact of feminism and poststructuralism. Beyond the literary charm and scholarly weight of this book lies an important attempt to provide a new model for general history.

Is it a success? As narrative history most certainly, but historians identified with newer trends have carped about its political focus, its neglect of the family, its author's preference for a revolutionary challenge to capitalism, and so on. None of these critics, it seems to me, appreciates either the method or the purpose of general history. Macintyre in fact uses on the title page a phrase omitted from the cover: "The succeeding age." His book is not just a narrative of events but a statement about the period: that it was too cautious in ideas, too negative in policy, too British in its view of the world, and above all too frightened by the social forces that had been mobilized in the preceding period. The sensibility behind this statement, as much as the knowledge embodied in it, are the product of a later "succeeding age," one in which historians discovered history-from-below but progressive intellectuals lost their faith in the masses. It is this affinity with our age that makes Macintyre's book such a resounding success as general history.

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IAN MCGIBBON. *The Path to Gallipoli: Defending New Zealand 1840–1915*. Petone, N.Z.: GP. 1991. Pp. xiv, 274. Cloth $49.95, paper $39.95.

New Zealand was the smallest and most remote of Great Britain's self-governing colonies. Exposed and vulnerable as it was in the South Pacific, issues of local and imperial defense were high priorities for its small population. In this meticulously researched and carefully crafted study, Ian McGibbon traces the evolution of New Zealand defense planning from the establishment of the colony in 1840 to the landing of troops at Gallipoli in 1915.

Until the 1880s the primary preoccupation of defense strategists was the preparation of local defenses, particularly gun emplacements at key harbors. McGibbon recounts the twists and turns in the debate over method and timing in great detail but clearly underlines the tension between the local politicians and strategists and the officials in London who of necessity had to husband resources as well as consider overall imperial needs. The result was often unsatisfactory and New Zealand's local defenses were of questionable merit after some fifty years of policy fluctuations.

The real impetus to major change and accomplishment occurred in the 1880s with a series of Russian scares and a general heightening of international tension. From then on Britain was under steady and increasing pressure from imperial and economic rivals, and New Zealanders had to accept that their own first line of defense was a strong and unified imperial force. Efforts were then concentrated on preparing an adequate contribution to both the military and naval defenses of the empire. New Zealand sent its first expeditionary force overseas during the Boer War of 1899–1902. This provided a considerable stimulus to general defense planning. In the years before the outbreak of world war in 1914 major changes were made in New Zealand's military and naval organization. Policy makers created a territorial force, introduced compulsory military training, devised a clear-cut naval policy involving contributions to the imperial navy, and streamlined the overall command structures.

McGibbon treats each development clearly and comprehensively and successfully brings out the tensions inherent in the relationship of a self-governing colony striving for a sense of identity and a presence on the international stage, and an aging imperial power insistently clinging to the traditional vestiges of authority. McGibbon is particularly adept at elucidating Great Britain's relative decline in naval strength and the impact that had on its fortunes internationally, as well as on Australia and New Zealand in the South Pacific. This is a good book that makes a significant contribution to both New Zealand and imperial historiography.

JOHN KENDLE
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DENYS LOMBARD. *Le Carrefour javanais: Essai d'histoire globale*. Volume 1, *Les limites de l'occidentalisation*; volume 2, *Les réseaux asiatiques*; volume 3, *L'héritage des royaumes concentriques*. (Civilisations et sociétés, number 79.) Paris: École des Hautes Études en Sciences Sociales. 1990. Pp. 263; 420; 337. 550 fr. the set.

In the introduction to this work, Denys Lombard comments that our European-centered historical vision regards only several non-Western traditions as impressive enough to qualify as "classical" civilizations: most commonly, China, India, and Islam. In contrast, the vast archipelagian territory that today stretches across Indonesia, Malaysia, and the southern Philippines is barely acknowledged as a cultural area, let alone one of the most remarkable laboratories for intercultural exchange in the world. The region's insular expanse, its fragmentation in modern times into separate nation-states, and its religious and ethnic diversity all convey an impression of cultural incoherence. For many scholars, it would seem, *l'Insulinde* is no more than an "exotic reserve and a lost continent" (vol. 1, p. 7), of little significance for comparative history.

Through three volumes, comprising some 1,000 pages, 2,400 footnotes, and 2,000 bibliographic references, Lombard attempts to correct this misimpression. In no other portion of the globe, he observes, can one see such a "juxtaposition or fusion" (vol. 1, p. 7) of almost all of the world's great civilizations. This is a region in which, from the fifth to fifteenth centuries, Indian civilization left its mark in religion, government, and the arts. From the thirteenth to the fifteenth centuries much of the local population converted to Islam, and Muslim and Chinese traders created a thriving network of merchant cities linked to India, China, and the Middle East. European civilization arrived shortly thereafter and eventually provided additional ingredients for this complex cultural mix. The result, Lombard explains, was not a region that developed in pristine isolation from other civilizations, but a "crossroads" (*carrefour*) deeply marked by the interaction of several traditions (vol. 1, p. 8). The example, Lombard observes, encourages us to "escape from the artificial notion of 'classicism,'" and to recognize that all great civilizations are in fact products of a similarly diverse cultural "brocade" (vol. 3, pp. 156–57).

Although his work abounds with insights into the history and cultures of the entire archipelago, Lombard uses Java, the area's most densely populated and influential island, as the vantage point from which to examine the cultural waves that swept through the region. The author devotes one volume to each of the most important influences, in reverse order of their appearance on the archipelagian scene: European, Muslim and Chinese (treated together due to the concurrent chronology of their arrival), and the Indic-Javanese heritage of inland Java. Lombard employs a loosely diachronic narrative to discuss each of

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these sociocultural "constellations," but he eschews what he terms the strict "corset of chronology" (vol. 3, p. 151). Instead, his presentation proceeds diachronically but, like a Javanese *gamelan* performance, regularly circles back on itself to gradually reveal a more complex pattern. Thus, for example, having introduced readers to the impact of European civilization on native Javanese kingdoms in volume 1, the author returns to the evolution of indigenous polity in volumes 2 and 3, each time deepening our understanding of the topic relative to the larger sociocultural "constellation" that is the focus of the volume at hand. Hence we come to understand native political institutions from the perspective of, first, advancing European power, then an earlier Muslim and Chinese presence, and finally the history and *mentalités* of the Indic-Javanese state itself.

This style of presentation violates conventional chronological narratives, but its juxtaposition of what were, in reality, often competing social complexes allows Lombard to explore their interaction without losing sight of the complexity internal to each. The work succeeds brilliantly, creating far and away the most comprehensive history of Java ever written and providing a feast of insights into cultural issues that no previous work, historical or otherwise, has explored in comparable detail. Whether discussing the impact of Western capitalism, the constricted growth of Christianity, the fitful development of Western education, or the "disarray" seen in modern Indonesian art (vol. 1), or whether discussing the growth of coastal towns, a monetized economy, Muslim boarding schools, transport, Chinese business firms, or linear concepts of time (vol. 2), or, finally, discussing Indic-Javanese styles of state administration, urban space, tax farming, status, etiquette, and personhood (vol. 3), the tapestry Lombard weaves is diachronically exhaustive and culturally rich. The author writes free of all fashionable jargon. And, although he comments that his is above all a "*histoire des mentalités*," he always brings "mentalities" back to the question of "which groups and which social cells were their primary bearers" (vol. 1, p. 9).

There are a few points on which readers may question Lombard's judgment. Some sociologists will find his concept of "occidentalization" too broadly cast. Are secular nationalist movements, which first originated in North and South America in opposition to Western European colonialism, properly understood as examples of Westernization when they take place in a non-Western context? Even if their ideologies and organization borrowed from Western models, as was the case in Indonesia, it can be argued that the "Westernization" involved in their genesis in the local context is secondary to the simple sociological fact that, in different times and places, similar political challenges give rise to similarly organized social movements. The same might be said of industrialization, bureaucratic government, and a host of other institutions of Western provenance.

Other readers may want to add to the author's analysis of agrarian society in volume 3. Lombard correctly laments the stereotyping of rural society that occurs in Java studies, and the lack of systematic historical or ethnographic monographs from the island's varied regions (vol. 3, pp. 15, 75). A more patient reading, however, of extant ethnographic sources—on par with the author's careful examination of historical and literary materials—would have enriched the discussion of rural Islamization in volume 2 and of non-conformist, antihierarchical traditions in volume 3. Although woefully incomplete, available ethnologic studies reveal that rural Java has long had religious, linguistic, and social traditions quietly resistant to courtly hierarchy and thus, in one sense, "preadapted" to the emancipatory message of a modernizing Islam.

These small points only complement the author's larger argument, the overall achievement of which is stunning. In its ability to move between intellectual history and political-economic process, this book takes exception to analytic fashions that segregate the mental from the material. Its insights into the development of indigenous capitalism in Southeast Asia, and its relationship to the state, Chinese mercantilism, and religious ideology are pathbreaking and promise to revolutionize scholarship on economy and culture in Southeast Asia. For the non-Southeast Asianist historian, Lombard's study provides a wealth of material for the comparative study of modern politics, economic change, daily life, and ideology, and does so in a manner informed by a profound appreciation of European and East Asian social history.

Readers accustomed to the abbreviated expositions fashionable in much social inquiry today should not be put off by the length or density of this work. This is, and I use the term deliberately, a masterpiece without peer in Southeast Asian studies and a brilliant achievement in Asian historiography. Lombard's work deserves to be read by all scholars interested in the comparative understanding of the modern world.

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UNITED STATES

Eric Foner, editor. *The New American History*. (Critical Perspectives on the Past.) Philadelphia: Temple University Press. 1990. Pp. xi, 292. Cloth $39.95, paper $16.95.

Save for periods of complete stagnation there has always been a new American history, or more likely several schools of it. They serve the profession by retiring outworn categories and concepts and bringing forward new fields and interpretations. Their advocates are often persuaded that they have "remade" or "redefined," or brought about "the transformation" of history—all claims made by the work at

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hand for the last twenty years of historiography. An additional demand for our attention appears on the title page of this volume: "Edited for the American Historical Association."

Editor Eric Foner has rounded up thirteen essays from as many historians, well informed and able, all of them. Seven of the thirteen address the subject by periods and six by "major themes." Foner admits some overlap of the essays (three of them, for example, take up slavery in some aspect) and he concedes that the collection falls short of comprehensiveness. The newness of the new takes numerous forms—subject, data, method, interpretation, emphasis, synthesis, sympathy, aversion—depending on the need or taste of the essayist. The newness most common to them is a shift of attention from the upper to the lower social ranks. The degree to which the new departs from the old differs widely from one essayist to another.

Perhaps the widest departure from the old is that of John M. Murrin on the colonial period. While the Enlightenment had viewed the discovery of America as a "hideous mistake," Murrin says that new scholarship "tells a story a good deal more dismal." The cost in native lives was perhaps triple the old estimate of twenty million, the main villains being microbes. Best estimates of pre-Columbian population of the Americas now exceed seventy million, as compared with fifty-five million Europeans "east" (surely he meant west) of the Urals. Traffic between hemispheres was largely an exchange of vices, with losers far outnumbering winners. The new history tends to stress the savagery of the civilized and the civility of the savage and to view early America as "a catastrophe—a horror story, not an epic" (pp. 4, 5, 7).

By way of contrast, Richard L. McCormick finds elements of old interpretation still applied to the period 1877 to 1917, a lack of new viewpoints to replace them, and no coherent synthesis as yet. In new work on the revolutionary generation, Linda K. Kerber reports a revival of the old Progressive focus on class conflict along with a less-conservative view of republicanism and less attention to great leaders than to the obscure led. Sean Wilentz deplores fragmentation by subspecialties and the denigration of politics and policy resulting from much of the new history and seeks in the market revolution a unifying theme for the 1815–48 period. In it he finds the ultimate paradox of the age, that having fostered a new mass democratic politics, the market revolution at the same time widened the gap between North and South and prepared the way for the Civil War.

On slavery, Civil War, and Reconstruction, Foner also regrets fragmentation by new research, but discovers an emerging synthesis that sees slavery as the cause of the war and emancipation as the central theme of war and Reconstruction. This is hardly the most striking innovation of the new history, but we benefit from Foner's authoritative assessment of the flood of recent scholarship in these fields. Revisions in interpretation of more recent periods tend to be less radical than evolutionary and to take the familiar form of challenges to dominant scholarship. Alan Brinkley's essay on the New Deal period, for example, traces the attacks on the interpretation by Arthur M. Schlesinger, Jr., attacks that stress failures, blunders, and conservatism of the welfare state and its reform. The period since 1945 is so recent that William H. Chafe's treatment is as much participation as assessment. He sees John F. Kennedy as a conservative on domestic issues and Lyndon B. Johnson as the most successful president of the postwar period in advancing social and civil rights legislation. Chafe's unastonishing conclusion is that the period witnessed both change and continuity.

Of the essays on themes the one on social history by Alice Kessler-Harris is notable for showing how the contemporary school, influenced by the *annalistes*, Marxists, and behavioralists, has challenged old notions of national destiny and national character. She fears, however, that social history, for all its merits, has romanticized ordinary folk, fragmented history, and lost direction by dissociation from politics. There is no challenging Linda Gordon's claim that productivity in women's history is unequaled by any other field in the last decade. Inspired by keenly felt grievance and neglect, women's historians, she says, have usually remained, as always, feminists of one kind or another, though not necessarily activists. African-American history was started but not monopolized by black historians, and Thomas C. Holt gives generous credit to contributions whites have made to the field in recent years. Historians of both races have greatly enriched understanding of black experience and race relations.

Like new historians of other fields, according to Leon Fink, labor historians have shifted focus to common folk instead of their leaders, to their daily life instead of their unions, to bottom-up instead of top-down history. The dynamism attributed to labor historians contrasts strangely with the precipitous decline of labor itself. Assigned the subjects of ethnicity and immigration, James E. Shenton does what he can—which is not inconsiderable—to add to what the previous essayists have already said on these subjects. On diplomatic history, Walter LaFeber ends the book with its most engaging essay on what must be the most explosively revisionary subject of all—the rise of a global empire that menaces constitutional freedoms it is supposed to protect.

Granting that everything could not be included, it is nevertheless regrettable that, considering the prominence they had in the period, space could not be spared for more on comparative history, regional history, and Marxist history. Even so, and despite other omissions, editor and contributors deserve a respectful salute for services rendered.

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ROBERT B. WESTBROOK. *John Dewey and American Democracy*. Ithaca: Cornell University Press. 1991. Pp. xix, 570. $29.95.

John Dewey, wrote Henry Steele Commager in *The American Mind* (1950), served as "the guide, the mentor, and the conscience of the American people: it is scarcely an exaggeration to say that for a generation no major issue was clarified until Dewey had spoken" (p. xiv). With the publication of this outstanding biography, Dewey's voice becomes clearly audible again. During his long lifetime (1859–1952), Dewey wrote important books on topics ranging from epistemology, ethics, aesthetics, and metaphysics to psychology, education, politics, and religion. His collected works run to thirty-seven thick volumes. Until Robert B. Westbrook, no one had undertaken the thorough analysis of Dewey's entire corpus of scholarly and popular writings. Thanks to Westbrook's meticulous scholarship, the most significant as well as prolific American philosopher of the twentieth century at last has a biography that does justice to his myriad achievements. At a time when decentralization of authority and increased participation are being offered as solutions to the problems facing America in education, industry, and politics, it is instructive to return to the ideas of this century's most resolute champion of democracy. It is also sobering to reflect on the obstacles he encountered everywhere he turned.

Westbrook concentrates on Dewey's public role as writer and activist rather than speculating on his psyche—although he does discuss Dewey's critique of Freud in one of the most interesting passages in the book. The details of Dewey's personal life are all here, including the tragic deaths of two of his sons and the romantic impulses that found expression only in Dewey's poetry. Yet this remains a study of ideas. Westbrook examines carefully and incisively Dewey's sometimes murky philosophical writings, which analytic philosophers have tended to dismiss but to which a new generation of pragmatists is now returning. As his title suggests, however, Westbrook focuses primarily on Dewey's social and political thought. Dewey was "a deviant among American liberals" (p. xvi), Westbrook writes in his preface, and he is at pains to distinguish Dewey from the various "corporate liberals," "democratic realists," "technocratic progressives," "Cold War liberals," and "liberal pluralists" with whom Dewey occasionally consorted. Although some historians will applaud Westbrook's rigorous insistence on Dewey's purity, others might conclude from the evidence that Dewey himself was more latitudinarian in choosing his allies than Westbrook is on his behalf.

Dewey's career unfolds in Westbrook's fully contextualized study as a series of responses to social, political, and intellectual controversies. Readers encounter Dewey in Jane Addams's Hull House and in the laboratory school he and his wife established at the University of Chicago, in behind-the-scenes maneuvering after World War I regarding the Polish government-in-exile and in the ferment of early twentieth-century China, in Philadelphia collecting and assessing art with Alfred Barnes, and in Mexico City debating Leon Trotsky's guilt. Westbrook just as skillfully reconstructs the less-dramatic but equally significant disputes between Dewey and his critics—Randolph Bourne, George Santayana, Lewis Mumford, A. J. Ayer, Bertrand Russell, and Herbert Marcuse, among others—and examines with particular care Dewey's long-running debates with Walter Lippmann and Reinhold Niebuhr. In his epilogue, Westbrook presents a richly informed and frankly partisan discussion of the uses and (in the case of Richard Rorty) abuses of Dewey by contemporary pragmatists ranging from Richard Bernstein and Cornel West to Jürgen Habermas. As that list should suggest, examining Dewey's career means examining a big slice of modern thought. Westbrook willingly admits that he thinks Dewey was sometimes unpersuasive or in the wrong (notably in defending the American cause in World War I and in the Cold War), but he also offers spirited defenses of Dewey from those who have misread or misappropriated him. Radical critics who treat Deweyan education as a means of social control are off target, Westbrook insists, but so are Dewey's self-styled friends in what passes for "progressive" education, which Dewey, with uncharacteristic bluntness, dismissed as "really stupid" (p. 503) inasmuch as it neglects the need for rigorous intellectual discipline.

Westbrook demonstrates that Dewey's philosophy rested on several central ideas. First, Dewey believed that democracy is an ethical ideal rather than merely a political arrangement. Second, he considered participation, not representation, the essence of democracy. Third, he insisted on the harmony between democracy and the scientific method: ever-expanding and self-critical communities of inquiry, operating on pragmatic principles and constantly revising their beliefs in light of new evidence, provided Dewey with a model for democratic decision making. As Westbrook points out, Dewey endorsed the physics of Albert Einstein and Werner Heisenberg precisely because they revealed that science, like philosophy, is a thoroughly human enterprise, reflecting human values and desires. Dewey's instrumentalism, unlike so much twentieth-century social science, rested on the irreducibility of human culture. Finally, Dewey called for extending democracy, conceived as an ethical project, from politics to industry and society.

If Dewey's greatest strength was his vision of democracy as participation, his greatest weakness was his inability to conceive strategies that would lead American culture toward that promised land. Westbrook notes that throughout his career Dewey became increasingly aware of the obstacles presented by entrenched power and alert to the intricacy of the problems facing modern cultures. But he did not

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succeed in formulating plans to resolve the conflicts that stand in the way of achieving democracy or the conflicts that persist even when participation becomes the order of the day. The same "unshakable faith" (p. 192) that sustained Dewey in his long crusade for democracy prevented him from appreciating fully the reasons for, and the depth and breadth of, the opposition to his ideal. If Dewey expressed the powerful and persistent American aspiration toward democracy, his critics have reflected an equally profound ambivalence about extending the principle of participation to all forms of social life. Everyone interested in probing this crucial tension in American culture will find Westbrook's splendid book challenging and rewarding.

JAMES T. KLOPPENBERG
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JOAN HOFF. *Law, Gender, and Injustice: A Legal History of U.S. Women.* (Feminist Crosscurrents.) New York: New York University Press. 1991. Pp. xi, 525. $39.50.

Joan Hoff frames her legal history of women using three presumptions. First, legal change—and in particular so-called egalitarian reform—has never been an accurate measure of progress for women. She calls this her "broken barometer" theory of history, and it bears strong resemblance to Derrick Bell's convergence theory of African-American legal history: those with power—whites in Bell's case, men in Hoff's—only give that which they want to give. Second, all women—regardless of race or class (or, a subject almost undiscussed in the book, sexual orientation)—should be understood as having continuously shared a common identity throughout American history. Third, struggles for equal rights have been a trap for women set by a male legal culture. Such struggles have usually blinded women to their own best interests.

Hoff's book argues strenuously with the more optimistic focus on women's legal agency and rights consciousness that has characterized much recent historical writing. The story she tells is thus intentionally pessimistic, although moments of possibility and transformation appear occasionally, as befits a work that honestly incorporates most all the recent writing on women's legal history and the legal and constitutional history of family relations.

There are brilliant moments in the book. I think, for example, of her perceptive reading of the Declaration of Sentiments (1848), as fundamentally concerned with marriage. Or, to take another example, her fine discussion of the changing place of dower rules in the law during the first half of the nineteenth century (followed, unfortunately, by an unconvincing quantitative analysis of dower and female wealth holding). She is, moreover, correct to place marriage and marriage law at the heart of women's legal history.

Yet because she insists on a separatist legal history, because she refuses to view marital rights relationally, as incorporating duties and obligations on the part of both husbands and wives, her discussion of family law is overall truncated and sometimes misleading. I suspect that she may have been captured by the very liberalism she criticizes throughout the work—at least that part of liberalism that imagines rights as possessions (or absences) rather than as relationships.

At the heart of the book stands a paradox. Women have been deeply alienated from the law; yet, in Hoff's rendition, women's status and identity has been derived from their legal rights. In spite of her generous appraisal of the work of Mary Beard, she misses Beard's central insight that the meaning of marriage for women cannot be captured by a list of the legal rights they possessed or lacked. To Hoff, the fact that women began to win child custody cases during the first half of the nineteenth century is no sign of positive change because women did not acquire legal rights to their children equivalent to those that men had held earlier. The facts that husbands lost what had once been presumptively theirs and that wives gained continuity in relationships that were crucial to them are apparently beside the point.

In its current incarnation, this is a deeply flawed work. But let me not be misunderstood. For the most part, the flaws are founded not in its perspective but in careless writing and extraordinarily bad editing. It gives every appearance of having been put together in too great haste. At times arguments in the text are incomprehensible without first reading the endnote. (See, for example, the passage on page 37, where one historian's "anachronistic" reading of Tocqueville is challenged.) The tables of laws affecting the legal status of married women are incomplete, unexplained in their categories, and unaccompanied with any identification of sources.

This book means to challenge the conventional whiggishness and optimism of our legal and constitutional history. Perhaps in a revised and reworked edition it will succeed in doing so.

HENDRIK HARTOG
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VIRGINIA LIESON BRERETON. *From Sin to Salvation: Stories of Women's Conversions, 1800 to the Present.* Bloomington: Indiana University Press. 1991. Pp. xvi, 152. Cloth $29.95, paper $10.95.

Virginia Lieson Brereton's survey of conversion narratives begins where one might expect in middle-class Protestant evangelicalism. She does not close her book, however, until she takes her readers into the discourse of twelve-step programs and feminist consciousness-raising groups. She read conversion narratives closely in order to describe a distinct genre; she ranges widely across two centuries in order to detect shifts in form and content. This ambitious and

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successful study is an important contribution to the historiography of women's experience and of religion in the United States.

Brereton outlines the typical nineteenth-century narrative. It began with accounts of sin, rebellion, and unbelief and moved to a description of the period of "conviction of sin," during which the narrators recognized their helplessness in the face of their propensities to sin. Conversion followed, and with it came a desire to be active, to labor for the Lord. After a lifetime of service and constant struggle against backsliding, the converted met death with serenity.

According to Brereton, these stories by and about women contain both "surface" and "submerged" plots. The surface plot is submission to God's will and to cultural expectations. The narrator's surrender to God "stands as an analogue of her obedience and submission to father, brother, or husband" (p. 28). Yet the submerged plot could enable women to stand up to men by claiming allegiance to God's will rather than men's. Since service in God's cause was the proper response to conversion, women often had a legitimate reason to leave the home for various kinds of religious and reform work. Also, the vivid and detailed descriptions of sin and rebellion gave women a chance to express and to experience vicariously emotions and emotional outbursts otherwise forbidden them.

Significant cultural changes left their mark on twentieth-century narratives. Brereton points to the marginalization of traditional Christian world view and evangelical vocabulary, the more rigid requirements of fundamentalism, the contributions of Holiness and Pentecostalism, changes in American English wrought by advertising and the media, a growing distrust of religious emotions, and the acceptance of therapeutic method and discourse. Twentieth-century women converts "describe lives of quiet desperation" rather than the crimes and rebelliousness of their predecessors. Recent narratives depict emotional pain and loneliness—an interior hell—more often than moral depravity and the fire and brimstone to come. In this century Jesus is a friend; he is a personal lord and savior.

Brereton concludes with descriptions of "out-of-church narratives," those "that begin in personal transformation" followed by "a fervent adoption of a political or cultural movement or idea" (p. 102). Despite radical discontinuities in content represented by these secular stories, the basic narrative structure is still intact—the movement from "an undesirable condition to a desirable one" (p. 103). And conversion, in Brereton's judgment, continues to offer wholeness and freedom. Critical to conversion for women in this culture is the yearning for acceptance just as we are by a powerful God, a loving Jesus, or a supportive community, be it Weight Watchers or lesbian sisterhood.

BETTY DEBERG
Valparaiso University

DAVID RUSSO. *Keepers of Our Past: Local Historical Writing in the United States, 1820s-1930s.* (Contributions in American History, number 129.) Westport, Conn.: Greenwood. 1988. Pp. xiii, 281. $42.95.

David Russo's previous books include *Families and Communities: A New View of American History* (1974) and *The Origins of Local News in the U.S. Country Press, 1840s–1870s* (1980). His newest book is thus obviously related to his earlier work. Russo's research is impressive and his treatment of the subject thorough. He insists that professional historians today owe much to the early antiquarians/amateurs who are the subject of his work. They were "the first great collectors and preservers of the artifactual and documentary evidence of the past in modern North America, and, without their strenuous efforts, much of the source material on which North American historical scholars base their research simply would not exist." Their efforts also "led to the founding of many of the historical societies, museums, and archives now managed by professionally trained staffs" (p. xii).

Russo divides his book into four parts: "The Early Antiquarians," "The Later Antiquarians," "Formulaic Local History," and "The Coming of the Academics." His focus is on "the general histories of towns and cities and on their authors" (p. 5). He also emphasizes New England, and claims that, at least for the pre–Civil War period (with the notable exception of John F. Watson, best known for his *Annals of Philadelphia*), most historical writing in the United States was confined to communities founded by the Puritans in New England. Nowhere else in the United States during these decades were there as many antiquarians who perceived the local past with as much seriousness (p. 75).

Ironically, while local history seems to interest many students, this book about local history is not likely to do so, for it is a difficult work to read. Russo makes the distinction between growing up in Deerfield, Massachusetts, where "the physical remains of the remoter past seemed to be everywhere," and his enrollment at Amherst, where the history he studied "was largely national and political in orientation" (p. xi). But it may be that he makes the relationship between academic historians and the antiquarians he studies sound a bit more confrontational than it really is, or at least than it need be. Still, his conclusion is thoughtful. "Academic historians have added analysis to description, generalizations to particularities, meaning to trivia, awareness of the essential characteristics of human life to simple storytelling," he writes. "Why can't they also search in their studies for the past's significance to their *own* lives? In this way, historical inquiry could become 'whole' again" (p. 213).

Russo is right: we do owe a great debt to the amateur historians he treats. We also owe him a debt for this thorough treatment of their work.

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East Central University

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WILLIAM G. MCLOUGHLIN. *Champions of the Cherokees: Evan and John B. Jones.* Princeton: Princeton University Press. 1990. Pp. xiv, 498. $39.50.

For nearly two decades, William G. McLoughlin has broadened our understanding of antebellum Cherokee society and, especially, of Cherokee and missionary cultural interactions and change. His newest book is the culmination of extensive research into federal, missionary, and tribal records. Focusing on the remarkable careers of Evan Jones (1788–1873) and John B. Jones (1824–76), father and son northern Baptist missionaries, McLoughlin presents a vivid and revealing analysis of the missionary role in Cherokee acculturation and political struggles that explains the continuing strength of the Baptist denomination among today's Cherokees.

Evan Jones and John B. Jones devoted themselves to improving the lives of the Cherokee people. They offered them, in the Cherokee tongue, a Christian theology that was "color-blind and treated all men as brothers, all women as sisters" (p. 5). By accepting a syncretic form of Christianity, Evan and John were able to convert more Indians to Christianity than any other Protestant missionaries. Through Evan the Cherokees obtained the Bible in their own language, and through John they obtained secular knowledge. Both men had their greatest success among the poorest Cherokees, the non-English-speaking, traditionalist full bloods, who constituted three-fourths of the tribe.

The Joneses served the Cherokees well. Evan, the only missionary to help the Cherokees resist removal after 1832, accompanied them on their 800–mile Trail of Tears to the West and lived a life of immense hardship and devoted service. In one of the more ironic turns of their careers, John (who had once been expelled from the Cherokee Nation for his opposition to federal policies) received appointment in 1870 as federal agent to the Cherokees. As agent, he helped them to resist congressional efforts to convert tribal lands into a federally controlled territory and to fight off invading Kansans, railroad speculators, and Texas cattle barons.

Although McLoughlin portrays the Joneses as champions of the Cherokees who assisted Indian resistance to white expansionism, he concedes that they were "not perfect heroes" (p. 7). Evan, an aggressive and dedicated evangelist, was often quarrelsome, domineering, disorganized, and prone to see himself as a victim of injustice. Although acquitted of murder charges stemming from a bizarre incident in 1833, his behavior left serious unanswered questions. John, like his father, dedicated himself to sustaining the dignity of the full bloods, but his commitment to bilingual education was so intense that at one point he was willing to accept federal control over tribal annuities and educational programs, thereby arousing the anger of even his strongest Cherokee supporters.

Generally, the Joneses promoted Cherokee nationalism and were drawn deeper into tribal politics. Both men worked closely with shrewd and charismatic National Party leader John Ross, the principal chief from 1828 to 1866, to defend Cherokee rights, and their lives became as heavily involved in tribal politics as in Christian preaching. Following the Civil War, they angered Ross by insisting that blacks be admitted as tribal citizens, but recouped their leadership role after his death in 1866.

Well written and researched, this book is not without some problems. McLoughlin, for example, mistakenly identifies Indian Office clerk Daniel Kurtz as the commissioner of Indian affairs in 1839 (p. 195) and claims that T. Hartley Crawford (who actually served as Indian commissioner from 1838 to 1845) was "replaced" as secretary of war in 1840 by Joel R. Poinsett (p. 201). The author's use of such pejorative terms as "barbarism" (pp. 34, 38, 49, 82), "heathen" (pp. 12, 119, 248, 299, 382), "pagan" (pp. 86, 380), "primitive" (p. 26), and "savagery" (p. 11), in reference to the Cherokees, without the use of quotation marks that accompany the same or similar words used elsewhere in the text, is regrettable. Finally, there are several places in the otherwise well-documented narrative where footnotes seem warranted but are missing (see pp. 158, 162, 229). Nevertheless, McLoughlin is to be commended for this insightful study of the remarkable careers of the Joneses and for reminding us that there were alternatives to acquiescence in "cultural injustice and oppression" (p. 484).

RONALD N. SATZ
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EDWIN R. SWEENEY. *Cochise: Chiricahua Apache Chief.* (The Civilization of the American Indian Series, number 204.) Norman: University of Oklahoma Press. 1991. Pp. xxiii, 501. $27.95.

Edwin R. Sweeney has written a magnificent work of historical biography. It is worthy to stand beside Angie Debo's *Geronimo: The Man, His Time, His Place* (1976) and Dan L. Thrapp's *Victorio and the Mimbres Apaches* (1974) as essential books in any biographical approach to the study of Apache history.

Sweeney's writing is precise, meticulous, and makes excellent use of a wide variety of documentary resources. He has managed to integrate in useful fashion Mexican and American official records as well as personal accounts and newspaper reports of events in Apacheria during his subject's lifetime.

In twenty chapters spanning five hundred pages, the author presents a panorama of Chiricahua Apache interaction with Mexican and American communities from the early years of the nineteenth century through 1874, the year of Cochise's death at the Chiricahua Apache Indian Reservation.

The book is strongest in its clear and well-written

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accounts of the relationships among Cochise's Chokonen band of Apaches and those communities whose officials and private persons left documentary materials presenting their versions of such encounters. He tells us much less of Chiricahua relationships with Pima, Papago, Zuni, Navajo, and other preliterate communities. Where a number of separate written accounts of an event exist, Sweeney notes and integrates them into an understandable "larger picture." In this respect, the book is superb and is unlikely to be surpassed.

Sweeney's biography is also strong in its careful analysis of structure of leadership among varied Apache bands. He extends his analysis to include the roles of leading women in negotiations between communities and decision making. He might have profitably expanded that element of his work still further.

The greatest weakness in Sweeney's excellent book is a failure to adequately center Cochise within his own culture. The author is cognizant of this as he writes of spiritual power among Chiricahuas: "Not being Apache, I would be the first to admit I do not fully comprehend its concept and significance" (p. xxii). For this reason, a reader would do well to have available Morris E. Opler's *An Apache Life-Way*.

Sweeney's attempt to amplify the Apache cultural perspective by a short description of the "Dikohe" war novice complex (pp. 10–14) is useful but insufficient. Sweeney appears to have made use of Chiricahua Apache sources whenever they were available. Those materials are not, however, integrated into the fabric of the narration as well as are his other materials.

The book is illustrated with period photos and contains a useful bibliography of works on and about his subject up to those published early in 1988. I highly recommend this fine biography to anyone interested in Apache history, whether scholar, specialist, or casual reader. It is superb.

D. C. Cole
Moorhead State University

Janet A. McDonnell. *The Dispossession of the American Indian, 1887–1934*. Bloomington: Indiana University Press. 1991. Pp. viii, 163. $20.00.

Native Americans have clearly recognized the close connection between their cultural identity and their land base. So have powerful whites. Janet A. McDonnell's brief book is a good account of a federal policy era that was an unremitting disaster for natives and an incredible opportunity for white land speculators. Between 1887 and 1934 Native Americans lost 86,000,000 acres—two-thirds of their remaining land base; much of what they had left was leased out to non-Indian entrepreneurs.

Federal policy in that period aimed to assimilate all Indians into American life. This was best accomplished, policy makers believed, by breaking up the common lands of reservations into individually held allotments, then helping each individual allottee to become a small-scale farmer or rancher. The legislative base for the assimilation policy was the General Allotment Act (Dawes Act) of 1887, but its implementation did not hit full stride until about twenty-five years later.

McDonnell plays up the resulting contradiction between policy ideals and policy implementation. The government scrimped on supplies and expertise; much of the best land passed into white ownership as impoverished allottees had their land titles suddenly thrust on them, often against their will; Indian agents conspired with local white farmers to rent Indian land for less than a pittance; the government squandered millions on reclamation projects that benefited non-Indian land, then tried to extract the cost from Indian allottees just when they were trying to start their own farming. McDonnell captures the theme of the shenanigans, but with understated emotion: "Putting the land into productive use became more important than protecting Indian ownership of that land" (p. 4).

The book's chapters each handle a facet of the land policy of the time: the allotment process, the leasing policy, water rights, determining "competency," and, near the end, the belated efforts to halt the alarming loss of Indian resources. McDonnell's focus is largely on Washington, tracing the actions of various secretaries of the interior and commissioners of Indian affairs as they tried to balance greedy demands of white economic interests against the trusteeship obligation to Indian communities. For much of the period there was no question of which position would prevail.

McDonnell's portrayal of the bumbling and self-serving efforts of federal officials will be familiar to those studying more recent federal policy: "The Indian Office relied on the 'integrity and good work' of its agents in implementing its leasing policy because it could not witness the situation in the field first hand" (p. 58). Others used virtually identical words to sketch a basic problem with Indian mineral and petroleum leases in the late 1980s.

Although the contradictions, racism, chauvinism, and unlawful expropriation come across, the author pulls her emotional punches and takes the color from her prose; the resulting "objectivity" at times is distracting. And it would have been good to see more references to the work of anthropologists. But these are minor quibbles. The book is a solid, informative account of a policy era that has received little scholarly attention compared to its successors. It should be required reading for anyone studying the dynamics of both federal policy and reservation life.

Robert L. Bee
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ALISON R. BERNSTEIN. *American Indians and World War II: Toward a New Era in Indian Affairs.* Norman: University of Oklahoma Press. 1991. Pp. xiv, 247. $21.95.

World War II fueled a spirit of unity and consensus that impacted all Americans. In this important new contribution to contemporary Indian affairs, Alison R. Bernstein analyzes how that experience affected Indians, tribes, and the direction of federal Indian policy.

When the Selective Service Administration brought the draft to reservations, some Indians fought the attempt to force them into fighting the white people's war. But in the end the principle of federal authority superseding tribal sovereignty prevailed, and most, although not all, Indians eventually joined in the patriotic fervor of the era, with many young men enlisting in the armed forces.

For some it was an assimilating experience. They prayed to the white god along with other soldiers and became attached to a steady income and the consumer culture. They liked the respect and equal treatment they received in the military, and they came to appreciate the whites' educational system that seemed to hold the key to continuing success. For the many Indians who left reservations to work in urban war industries, the effect was similar. Detribalization and acculturation resulted, but the fact that so many moved to cities near reservations and lived a kind of in-between cultural existence testified to the persistence of tradition.

Ironically, the wartime consensus came shortly after Commissioner of Indian Affairs John Collier had boldly attempted to reverse policies of assimilation and chart a new bilateral relationship between tribes and the government in the Indian Reorganization Act of 1934 (IRA). Indian veterans challenged its celebration of separatism and tribal rights. They used the language of the emerging civil rights movement to fight against forms of discrimination like bans on alcohol and the lack of voting rights and to agitate for the removal of restrictions on trust lands. White conservative opponents of Collier, however, turned this into a wholesale attack on tribalism, the IRA, and the Bureau of Indian Affairs in an obvious precursor to the termination policies of the 1940s and 1950s.

Bernstein argues that the war experience also gave rise to the more recent movement for self-determination in Indian communities. The generation of Indians who came of age during the war knew more of the white world and were familiar with constitutions, legal proceedings, and majority rule. But they did not forget traditional roots and worked to defeat termination when its white supporters pushed it too far. They indeed came out of the war with goals of winning the rights of American citizenship, but not at the expense of tribal rights.

Scholars will welcome this book as a helpful aid in understanding recent Indian affairs. It is generally clear and well written. It will undoubtedly stimulate debate over the impact of wartime nationalism on Indians as well as over the roots of policies such as termination and self-determination. But as insightful as it is, some will likely find the case for World War II as the impetus behind nearly every modern development overstated. Could the same experience shake the foundation of tribalism and cultural pluralism, lay the groundwork for termination, and still give birth to the modern notion of self-determination? Maybe so, but perhaps the IRA, the Red Power movement, and larger national and international developments like the civil rights movement and the indigenous peoples' rights movement deserve as much consideration in explaining self-determination.

LARRY BURT
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WILBUR R. MILLER. *Revenuers and Moonshiners: Enforcing Federal Liquor Law in the Mountain South, 1865–1900.* Chapel Hill: University of North Carolina Press. 1991. Pp. xii, 251. Cloth $34.95, paper $12.95.

If historians no longer tell us "how things actually were," some at least do tell us how things actually worked. That is what Wilbur R. Miller does in this careful study of the administration of the excise tax on liquor (1862) from its Civil War origins until the adoption of national prohibition, principally in the mountain South. But he also tells us what it meant that things worked this way and not another. He thus provides a new understanding of how the nationalization of American life actually occurred during the late nineteenth century and, not incidentally, how the southern mountain region—where violation of the whisky tax laws was most frequent if not most flagrant—became identified as an exception to the rule of national unity and homogeneity.

Originally conceived as a "painless" source of emergency revenue, the whisky tax, as extended by subsequent legislation and implemented in practice, required the creation of a national bureaucracy—the Internal Revenue Service—to collect it; involved the national government in its first attempt at regulating all phases of the manufacture, storage, distribution, and sale of a product, including the necessity to "inspect" and to "enforce" by punishing violators of the law; and effectively established the primacy of federal over state authority throughout the United States, even in those southern states that, conventional wisdom now tells us, were largely "let alone" during Reconstruction, and certainly afterward.

None of this was easy, and Miller tells the story with due attention to the complexities of law enforcement and administration, including conflicts among federal agencies and officers (IRS inspectors and collectors, U.S. marshals, district attorneys and judges, and army officers) in the field and in Washington, and to the often difficult relationship of federal agencies and

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officers with state and local agencies and officers (sheriffs, governors, legislators, judges, jailers, and juries). He also notes the impact of administrative style (Commissioner Green Berry Raum successfully sought to coordinate enforcement activities from Washington, but others placed more reliance on local collectors), the periodic revisions of the tax rate by Congress (it was easier for distillers to pay fifty cents a gallon than risk seizure of equipment and a jail term; at $1.10 a gallon there was so little profit for small producers that "moonshining" became increasingly popular), the changing "platforms" of the political parties (Republicans generally defended the tariff as an adequate source of revenue and promised to repeal all excise taxes), and the growth of "prohibition" (when counties, and then states, went "dry," moonshine liquor was the only liquor there was, and the business of the revenuers changed from tax-collection to the enforcement of morality).

How enforcement occurred is what this book tells us, in delicious detail; but it did occur and the result was that illegal distillers, whether "members of the underworld" (in Brooklyn) or "moonshiners" (in Appalachia), entered America's jails and America's fiction at once, the "primitive rebels" of a modern and modernized nation.

HENRY D. SHAPIRO
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CHRISTIAN G. FRITZ. *Federal Justice in California: The Court of Ogden Hoffman, 1851–1891.* (Law in the American West, number 1.) Lincoln: University of Nebraska Press. 1991. Pp. xv, 324. $40.00.

GORDON MORRIS BAKKEN. *Practicing Law in Frontier California.* (Law in the American West, number 2.) Lincoln: University of Nebraska Press. 1991. Pp. xvii, 192. $35.00.

The legal history of the American West, apart from episodes of vigilante justice and progressive political reform, remains largely unexplored by scholars. These two exemplary studies of legal change in nineteenth-century California, which relate institutions and doctrines to the larger problems associated with the rise of an urban-industrial society, thus fill a genuine historiographical need.

Both Christian G. Fritz and Gordon Morris Bakken take what might be termed a "personalist" approach to legal history. They emphasize the human face of professionalism by paying greater attention than most commentators to the role of clients and litigants in the shaping of the law. Fritz's book offers a uniquely valuable analysis of the work of a federal trial court over a forty-year period. Based in part on systematic research in the case files and docket books of more than 19,000 cases, it provides fresh insights into the nature of the judicial function and the social classes served by the court system. Although a cautious and conservative man by nature, Judge Ogden Hoffman acted creatively to fashion novel remedies for plaintiffs in admiralty suits, the largest group of cases to come before his court. Injured passengers, abused seamen, and merchants claiming property damage regularly prevailed against powerful shipping interests. These results, Fritz points out, contradict the assertions of critical legal studies scholars that nineteenth-century judges were staunch supporters of business and subsidized entrepreneurial growth through their decisions. Shipowners, who might have been expected to oppose Hoffman's rulings, supported them instead for their logic and predictability.

Hoffman was not a judicial activist in any consistent way, however. When a precedent had been clearly established, he followed it, regardless of his personal feelings. In a characteristic early case involving the "fellow servant rule," a major defense against employer liability for workplace accidents, Hoffman vigorously attacked the rationale behind the rule but nevertheless upheld its application against the injured plaintiff. His decisions in other areas of the law, such as criminal prosecutions and land disputes, likewise reflected a conscious effort to be impartial within the constraints imposed by precedent. Despite his acknowledged prejudice against Asians, he braved community anger by championing the civil rights of Chinese litigants threatened with deportation under increasingly repressive federal laws. His stubborn independence owed much to his background, training, and political values, Fritz concludes, but more important was his sense of judicial duty: as the sole district judge for Northern California, he identified his personal reputation with that of his court.

Bakken's social history of nineteenth-century California lawyers effectively complements Fritz's court-centered study. Like Fritz, Bakken rejects a critical legal studies perspective, with its emphasis on the manipulation of republican ideology by a pro-business professional elite. Instead, he finds the source of legal values in an autonomous rule of law that encodes the traditional moral norms shared by the American people. Through extensive research in the papers of individual practitioners and early law firms, he reconstructs the attorney-client dialogue and the underlying cultural assumptions that guided litigation strategy. The "law-mindedness" of the average Californian, he argues, even extended to many instances of vigilante justice, in which accused persons were given an opportunity to prove their innocence in an informal trial that mimicked the procedures of the regular criminal courts.

Based on a statistical sampling of 1,168 elite attorneys whose biographies appeared in bar, county, and city histories, Bakken's study examines the careers of two generations of California lawyers and draws useful comparisons with their counterparts in Massachusetts, Texas, and Kansas. The frontier phase of lawyering, in which debt collection and real-estate practice predominated, ended by the 1880s. Thereafter tort law and corporate practice assumed major

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importance, as urban lawyers turned increasingly from litigation to corporate counseling.

The arguments of Fritz, a law professor, and Bakken, a history professor, are persuasive in light of the voluminous evidence they have gathered. The representativeness of their findings must remain in doubt, however, until historians probe with equal skill into the legal institutions of other states.

MAXWELL BLOOMFIELD
Catholic University

H. L. POHLMAN. *Justice Oliver Wendell Holmes: Free Speech and the Living Constitution*. New York: New York University Press. 1991. Pp. ix, 265. $45.00.

MARK A. GRABER. *Transforming Free Speech: The Ambiguous Legacy of Civil Libertarianism*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xi, 336. $39.95.

Taken together, these books indicate that debates about the meaning of free speech in the American Constitution and the best way to ensure free speech in the American polity reflect the historical moment during which they occur, and that an understanding of that history is necessary if the right of free speech is to remain meaningful.

H. L. Pohlman's work is an attempt to reconcile Oliver Wendell Holmes's positivism, which defined law as no more than the governmental commands and societal preferences of the moment, with his defense of free speech against both government and the majority. He finds consistency in what he sees as Holmes's application of the doctrine of legal liability to the sphere of speech. Although Pohlman properly concedes that his thesis is not original, he attempts to break ground by demonstrating that just as Holmes's theory of legal liability had two branches and thus varied according to the case before him, so Holmes employed the "clear and present danger test" only in certain instances. Specifically, he eschewed it in the absence of illegal advocacy, where the speaker's intent was unlawful and there was proximity between his speech and a harm that society had a right to make illegal.

Mark A. Graber would argue that this misses the point, which is defining under what circumstances advocacy can be declared illegal by the government. (The scholar interested in the development of Holmes's thought might compare Pohlman's thesis with Graber's, especially pages 108–13.) What the two authors share, however, is the notion of free speech as an evolving concept, with Holmes's interpretation based firmly in a specific historical era.

Graber's book is primarily an attack on Zechariah Chafee, whose most important work he calls "a celebration of Justice Holmes" (p. 132). Graber argues that Chafee's view of speech as a prerequisite of democratic government rather than an aspect of individual liberty constituted a transformation of the theory of speech that is now itself ripe for transformation. He persuasively places Chafee's theory at the center of the Progressives' effort to differentiate between what they saw, on the one hand, as the early twentieth-century Supreme Court's mistaken protection of property from the attempts of reform-minded legislatures to limit its use, and, on the other, as the need to protect speech from the same legislatures. Thus, just as Holmes argued for somewhat different reasons, the Progressives claimed that economic policy should be left to the legislatures, but democracy necessitated the courts' intervention to protect speech.

Graber cites John Dewey's statement that the best any theory can do is clarify and help resolve contemporary problems, and views twentieth-century American civil libertarianism as a response to historical circumstance. Now, he argues, it is time to consider new social facts; in particular, the ability of money to affect political campaigns and policy. The question at the end of the twentieth century is no longer "Is money speech?" but "When is money speech?" His answer is that money, and its use, should not be protected speech when it is used to dominate political expression. He therefore disagrees with traditional civil libertarians who applauded what he sees as mistaken recent Supreme Court decisions limiting the power of Congress to prevent large entities such as corporations from using their wealth to dominate political discourse in the era of expensive electronic media. Graber calls for a reexamination of the doctrine of commercial speech and suggests that corporations should not have the same right as individuals to contribute to political campaigns.

Graber's thoughtful analysis of the transformation of First Amendment jurisprudence and his suggestion that it is no longer useful merit serious consideration. If Dewey was right, a reexamination of First Amendment theory is past due. Graber promises to undertake one in his future work, expanding on a conception of expression rights that draws on Michael Walzer's egalitarian political theory.

The only regrettable element of Graber's insightful work is his misunderstanding of the First Amendment jurisprudence of Louis Brandeis—a failing shared by Pohlman, who claims Brandeis misread Holmes's version of the clear and present danger doctrine when in fact he disagreed with it. Graber states that Brandeis implied in the case of *Whitney v. California* (1927) that "federal and state elected officials should have the power to act when they determined that the exercise of free-speech rights threatened other important social interests" (p. 103). To the contrary, Brandeis specifically rejected even "some violence . . . or destruction of property" as grounds for suppression of speech, with only the clear probability of serious injury to the state itself sufficing. Were Graber to look more closely, he might well find an ally rather than an enemy in Brandeis, who fought the very concentrations of wealth that Graber sees as monopolizing political discourse today. One hopes

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that Graber will revisit Brandeis before producing what should be a work as important as his current one.

PHILIPPA STRUM
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SHULY RUBIN SCHWARTZ. *The Emergence of Jewish Scholarship in America: The Publication of the* Jewish Encyclopedia. (Monographs of the Hebrew Union College, number 13.) Cincinnati, Ohio: Hebrew Union College Press. 1991. Pp. xii, 235. $35.00.

The twelve-volume *Jewish Encyclopedia*, published by Funk and Wagnalls between 1901 and 1905, was the first encyclopedic repository of information on Jews and Judaism. Although much of its material is now dated, its most outstanding articles are still useful. But whatever the enduring legacy of its contents, that work represents a landmark achievement, revealing a glorious chapter in American Jewish cultural history. Not only did it serve as the standard model for future encyclopedias on the Jews but, as Shuly Rubin Schwartz argues so persuasively in this penetrating and superb study, the *Jewish Encyclopedia* witnessed American Jewish intellectual leaders asserting their intellectual independence from their European Jewish peers, even as they incorporated the European tradition of *Wissenschaft des Judentums* into the encyclopedia, in order to promote an accurate understanding of Jews and Judaism. Disseminating accurate knowledge about Jews had a clear social aim: the organizers of the *Jewish Encyclopedia* hoped that its (supposedly) critically detached submissions would foster both a diminution of anti-Semitism and positive intergroup relations, as readers became aware of the multitextured and enviable historic Jewish culture.

Schwartz's book is divided into two broad sections. The first presents a wonderful reconstruction, based on rich archival sources, of the process by which the *Jewish Encyclopedia* came into existence. The author's account of what could have been boring "institutional" history is actually riveting, as she attributes the genesis of the Jewish encyclopedia idea to a wildly idiosyncratic European Jewish literary maverick, Isidore Singer, who, with his immigration, brought his concept to America. Schwartz painstakingly delineates the concretization and transformation of Singer's dream, which witnessed the involvement of Christian publisher Isaac Funk; the assembly of a frequently changing editorial board featuring an uneasy and ultimately stormy alliance among preeminent American Jewish rabbis, denominational thinkers, laymen, and scholars who constantly bickered; and the bureaucratic and financial tensions that threatened the project continuously. Indeed, given the explosive internal dissensions, pitting the publisher against the board and board members against each other (for example, Kaufmann Kohler and Emil Hirsch against Cyrus Adler and Solomon Schechter) and the presence of a petulant managing editor (Singer) whom nobody trusted, it is remarkable that the *Jewish Encyclopedia* project did not self-destruct, especially after the publication of volume 1 in 1901.

The second section of Schwartz's book offers a trenchant content-analysis of the encyclopedia's articles, evaluating carefully the major thematic categories of the work: history, biography, sociology, anthropology, literature, and theology. The author's critical analysis is replete with nuanced insights as to how Jewish contributors sometimes subtly yet consistently portrayed Jews in the most positive light, as well as the abiding relevance of Jewish traditions, and how the editors finessed difficult and divisive intellectual issues, such as the place of critical scholarship in elucidating biblical and rabbinic texts. This unit astutely combines the methodological approaches of intellectual history with a history of ideas, and does so with conceptual clarity and literary flair.

Schwartz has written a first-rate history. Her book manifests scholarly breadth and depth, a command of relevant primary sources and secondary works, and insight on Jewish intellectual history at the turn of the twentieth century. If she overreaches and claims too great an impact for the *Jewish Encyclopedia* on American Jewish culture—while useful to scholars, it did not revitalize Judaism nor did it offer a compelling paradigm of Jews rising "above their differences to work together" (p. 173)—in all other respects her study succeeds admirably and constitutes an important contribution to the history and historiography of American Jewish scholarship.

BENNY KRAUT
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JAMES M. WALLACE. *Liberal Journalism and American Education, 1914–1941*. New Brunswick, N.J.: Rutgers University Press. 1991. Pp. xiv, 237. Cloth $40.00, paper $15.00.

In this book, James M. Wallace discusses the liberal progressive critique of American education as found in *The New Republic* and *The Nation* from 1914 to 1941. Wallace describes the journals' opinions on educational topics of the day, such as the efficiency movement, school reform, teachers' unions, academic freedom, workers' education, and student organizations of the 1930s. He affirms the centrality of education to progressive visions of more general social change, and asserts the journals' importance in maintaining a consistent liberal voice, even during such politically conservative times as the 1920s.

The volume reads well, and it is also a pleasure, as David Tyack remarks in the book's introduction, to hear the eloquent and compelling arguments of Randolph Bourne, John Dewey, Walter Lippmann, Agnes De Lima, and others. Moreover, Wallace

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makes an important contribution by demonstrating that American educational debate in the twentieth century persisted between the better-known period of educational reform during the Progressive Era and the discussions of the 1950s and 1960s.

Wallace explores a promising area of scholarship: the relationship between journalism and social action. And he correctly notes the difficulty of tracing "ideas directly from the journals into people's minds and then into visible social change" (p. 168). Yet he might have analyzed this relationship more fully and creatively by placing his description of the journals' contents with broader contexts, such as the history of journalism, liberal ideology, and educational reform itself.

Readers would benefit from a chapter on the journalistic landscape in educational reform. During the Progressive Era in particular, muckrakers wrote extensively about education, and additional periodicals took a liberal progressive stance on the issues. Wallace's argument about the importance of *The New Republic* and *The Nation* might have been strengthened had he showed how they compared to other publications writing about education in the 1910s, 1920s, and 1930s, and had he provided a clearer sense of circulation figures and who read what. Similarly, Wallace avoids a larger discussion of liberalism. This omission means that readers do not hear about issues of race (briefly alluded to), gender (relegated to a footnote in the chapter on Agnes De Lima), or ethnicity (although unmentioned in the book, during the 1920s these journals reported on anti-Semitism in higher education) in educational debates.

Finally, although historians of education must analyze differences between policy and practice, ideas and reality, debate and action, this book fails to do so. Borrowing Michael Walzer's term, Wallace calls readers of liberal journals "connected critics" who acted on their beliefs, but he offers insufficient evidence that this was the case in twentieth-century educational reform. In 1940, James Wechsler left *The Nation* due to "that ancient disease of liberal journalism—the feeling that we were talking to ourselves, and to those who agreed with us" (p. 145). Unfortunately, Wallace does not assess the significance of this widely held view for his own argument about the role of the journals and the overall impact of liberal progressivism on mid-twentieth-century American education.

Lynn D. Gordon
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Wiley L. Housewright. *A History of Music and Dance in Florida, 1565–1865*. Tuscaloosa: University of Alabama Press. 1991. Pp. xvii, 448. $49.95.

Wiley L. Housewright has compiled a "narrative of music choices, music events" (p. 356) of the early history of Florida. Using government and church documents, letters, newspapers, and sheet music, Housewright argues that musical production and consumption were integral facets of the early settlement and history of Native American, Spanish, English, and American Florida. The lens of the author's investigation is trained on music in an attempt to connect cultural events to the broader sweep of historical narrative. Because the focus is on discrete examples of musical production between 1565 and 1865, however, this book is more useful as a reference than an analysis of the relationship of culture to history.

Housewright's chapter on "Music and the Indians" establishes a dichotomy between Florida's first settlers and later Spanish "discoverers." The Native Americans were "nonliterate," and the author relies on "European eye-and-ear witnesses, explorers, official historians, priests, settlers, and visitors" (p. 1) to document their musical history. The emphasis throughout is on official sources, although the author recognizes that the triumph of literate European over nonliterate Native American culture constitutes a loss that can never be fully recovered.

Housewright also recognizes the cultural divide between Native American culture and American sensibilities. For example, during the Seminole Wars, Major General Alexander Macomb first described the singing at a local Indian festival as "monotonous but agreeable," but only a few weeks later said that the same music comprised "hideous shouts and yells" (pp. 13–14). We have no clue as to why the officer's reaction shifted so dramatically. Incidents such as these are far from trivial in helping to illuminate Native American/white relations. An attempt to explain, or even to speculate, as to what other factors, such as growing hostility to the native population, may have influenced Macomb's musical judgment would help to connect culture to history in a more meaningful way.

The author occasionally cites musical examples that revealed political sentiments. Florida did not join the other British colonies in the revolution and was a haven for Loyalist refugees. The surprise singing of "God Save the Thirteen States" to the tune of "God Save the King" by three imprisoned Americans on July 4, 1781, marks the beginning of a change in political thinking that would culminate in Florida's entry into the union in 1845.

The presence of hymnbooks and musical instruments did not always produce elevating results, according to arbiters of high culture. Housewright tells us, for instance, that Andrew Jackson's wife Rachel was "an unwavering Presbyterian who lost no time in ridding Pensacola streets of fiddling, dancing, cursing, and gambling seamen" (p. 139). The preference for proper church music was reflected in hymn collections in Florida churches whose compilers advertised that they contained "valuable foreign musick . . . secure from the touch of American innovation" (p. 149). High culture in Florida, as in other parts of the United States in the nineteenth century,

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made an obligatory bow to Emerson's "courtly muses of Europe."

Housewright's accomplishment here is to present the diversity of music in Florida's history. From Native American chants to transplanted white and black church music, popular songs before and during the Civil War, instrumental music, and spirituals, he has mined the sources of Florida's musical history effectively in an engaging story of cultural interaction and creativity.

BARBARA L. TISCHLER
Columbia University

JAMES R. PERRY. *The Formation of a Society on Virginia's Eastern Shore, 1615–1655*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1990. Pp. xii, 253. $32.50.

This monograph on Northampton County, Virginia, provides the earliest community study yet available for the seventeenth-century Chesapeake. James R. Perry has used the eastern shore's county records to identify early settlers, locate their landholdings, and probe the nature and strength of the ties that bound them together.

Perry provides a series of maps to outline the gradual process of expansion along the county's bayside, beginning with grants made during the company period, then proceeding by decades from 1635 to 1655. His most important finding, one which underpins his argument for the primacy of community values over chaotic individualism, is that newcomers settled alongside previous occupants rather than leapfrogging over ungranted land. Perry searched for, and found, numerous ties between many of the new arrivals and their predecessors, but the salient discovery is an apparent preference to live near others.

The lack of church records, private correspondence, and diaries in this period of Virginia's history sorely tests the historian's skills. The forced reliance on county records could lead one to overstate antisocial behavior or economic matters, given their menu of crimes, suits for libel and debt, wills and inventories of estate. Perry, however, quite deliberately employs them to map spatial distributions of family, neighborhood, and economic relationships. He extracts from his materials a history of transactions that, he argues, demonstrates a thickening web of interdependence among these early Virginians.

Turning from these internal matters, Perry sketches the county's exterior economic and political relations before 1655. Especially important for his argument concerning the priority of local ties is the extraordinary behavior of the county court toward the central government at James City in the wake of the colony's capitulation to Parliament in 1652. Commissioners and "inhabitants" attending the court's meeting protested "excessive" taxes and declared independence, proposing to choose their own magistrates annually. Northampton won substantial concessions from the assembly and relinquished its delusions of autonomy.

In stressing the power of local ties, Perry joins a growing chorus of revisionist historians who disagree vehemently with the "Wild West" image of the tobacco colonies painted by those who stress the centrifugal forces at work in the region. Although life may have been as short and wealth as unequally distributed on Virginia's eastern shore as it was elsewhere in the Chesapeake, questions about the area's typicality persist. Other problems plague the work: its focus on white landholders obscures the presence of servants, blacks, and Indians, while the arbitrary ending date of 1655 makes one wonder what is missing. The book, moreover, lacks a systematic description of the contents of the court records, which prevents the reader from judging the thoroughness of the research and the caliber of the interpretation. Nevertheless, Perry is persuasive that propertied Virginians had sufficient interests in common to encourage them to seek closer community ties, guard their personal reputations, and support sanctions against unneighborly behavior.

GLORIA L. MAIN
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FIRTH HARING FABEND. *A Dutch Family in the Middle Colonies, 1660–1800*. New Brunswick, N.J.: Rutgers University Press. 1991. Pp. xviii, 326. $49.00.

Historians stumble onto research topics in any number of ways. The discovery of hitherto unutilized documents or the guidance of a mentor may stimulate some to fashion a manuscript-length study; others rely on simple intellectual curiosity, determined to expand the canons of knowledge or to craft a revisionary statement that turns previous scholarship on its head. All usually feel some personal interest or identification with their topic. Firth Haring Fabend can claim an even more intimate, familial interest than most: she is writing about five generations of her ancestors, the Harings.

This book is more than a mere chronicle of one's ancestors; it represents a serious attempt to understand how one particular Dutch family in Tappen, New York, created a life for itself and its descendants when not responding to and reeling from the turbulent currents of political, economic, and religious upheaval—staple features of middle colony life. Early generations of Harings concentrated on developing the Tappen patent west of the Hudson River, and they stood poised to defend their land titles from legal challenges and boundary disputes. Later generations dealt with the disruptions of the American Revolution, which ravished their property and shat-

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tered family loyalties. For all generations the family circle proved almost inescapable. Clustered together geographically, members frequently intermarried and developed a sense of kinship that was further enhanced by worship in the Tappen Dutch Reformed church. Intertwined with this familial network was a sense of being Dutch. How one defines "Dutchness" (the author's term) and how this was expressed by different generations of Harings forms a running commentary in the book. A family that sometimes resembled other middle colony farm families in their pursuit of profit could nevertheless retain Dutch cultural forms, reverting to memories of this earlier heritage.

Fabend develops this argument cogently and carefully. Her conclusions are usually well founded. Deprived of literary sources that would fully detail the emotional complexities of Dutch family life, she relies on wills, deeds, church registers, and court records to craft her thesis. Particularly interesting in this regard is Fabend's argument about the reemergence of Dutch cultural forms; although later generations of Harings had immersed themselves in Anglo-American culture, they reverted to older, Dutch architectural forms after the revolution, possibly to remind themselves of a mythical Dutch golden age before English rule in the province. Obviously, such an interpretation remains difficult to prove but it should inspire others to test her theories in subsequent research.

There are some weaknesses in this book. Occasionally, the author goes too far in explaining what the Harings thought or felt when the evidence cannot provide such statements. Most Harings may have been patriots, but whether this was due to Enlightenment ideas or Dutch religious heritage or both cannot readily be determined. When discussing the impact of trade boycotts prior to the revolution, Fabend relies on the letters of an English family in a similar economic situation to her subjects, people whose thoughts and concerns may or may not have paralleled those of the Dutch Harings. Even so, the strengths outweigh the weaknesses of this generously illustrated, well-crafted product. Students of the Dutch colonial experience should be pleased with the result.

Robert E. Cray, Jr.
Montclair State College

Kinloch Bull, Jr. *The Oligarchs in Colonial and Revolutionary Charleston: Lieutenant Governor William Bull II and His Family*. Columbia: University of South Carolina Press. 1991. Pp. xvi, 415. $29.95.

A descendent of William Bull II, Kinloch Bull, Jr., spent most of his adult life in the U.S. foreign service. On leaving government in 1985 he devoted himself to furthering his interest in history. The resultant study rests on extensive archival research and an appreciation of secondary works.

The first William Bull came from England to Charleston at the founding of the city in 1670 and by the middle of the eighteenth century his descendants were among the six families interrelated by marriage that dominated the Council of South Carolina. In addition to membership in the upper house, William Bull II (like his father before) was lieutenant governor and, during frequent and sometimes prolonged absences of the royal governor, served as acting executive. This book offers an opportunity to examine not only the causes of the revolution in South Carolina but also the social and political structure of the province. Unfortunately the author offers little on social structure, race relations, or planter life in South Carolina, a colony where over half the population was black slave. He does point out, however, that during military crises the need to maintain militia patrols to guard against servile insurrection or desertion severely limited military contributions. The treatment of politics and the causes of the revolution closely follow the familiar views set forth by Jack P. Greene and M. Eugene Sirmans. The story is one of institutional conflict, a struggle by the Commons House to retain rights and privileges against a royal government seeking to reassert former prerogatives. Rather than providing details of the revolutionary leaderships as Bull himself did in a long letter to London in 1770, the author falls back on vague formulae; for example, as the crisis with England developed in 1774, a "restless, sometimes inchoate revolutionary force was swirling through the capital and through the colony" (p. 196).

Yet readers may find another explanation for political opposition. A generation before, the Council was still the stronghold of the local political establishment, an upper house dominated by a few interrelated families, among them the Bulls, Middletons, and Draytons. Increasingly, the author speculates, but without many particulars, the crown, to the disgust of the Charlestonians, turned to royal placemen, strangers of low station who were sent out to fill up the Council. To what extent were places of honor denied the emerging leaders in the Commons? Perhaps the domination of the six great families of oligarchs was of greater weight; in a society where personal honor possessed a "sort of sacred quality even to be bypassed in order of precedence in the Council was considered a mark of dishonor" (p. 153).

According to Bull, the lieutenant governor's larger service to the royal administration was to keep matters quiet as the opposition elements mounted their attacks on the British authority. But in so doing did the lieutenant governor not allow the dissidents to win the minds of the populace by default? Perhaps the leader of the local Sons of Liberty made a more accurate assessment. Christopher Gadsden, a rising young merchant whose election credentials to the Commons Governor Thomas Boone had challenged,

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wrote that William Bull II in private life was an agreeable man, but as provincial executive "always had been the weakest and most unsteady man I ever knew" (p. 132).

Perhaps the most valuable aspect of this book is the appreciation of what happened to the once-dominant oligarchs during the revolution, of how the crisis splintered the class of great planters and the Bull family.

J. M. Sosin
University of Nebraska, Lincoln

Michael J. Crawford. *Seasons of Grace: Colonial New England's Revival Tradition in Its British Context.* (Religion in America Series.) New York: Oxford University Press. 1991. Pp. xi, 354. $39.95.

In this work Michael J. Crawford examines revivals and the meaning of revival in the English-speaking world during the seventeenth and eighteenth centuries through an analysis of discourse about revivals and the shaping of behavior by that discourse. He surveys ideas about revival going back to 1660 and shows how those ideas became expectations that bore fruit in revivals throughout the eighteenth century in Britain and the American colonies. A final chapter relates the later patterns of nineteenth-century revivalism to these earlier developments.

Crawford claims that throughout the eighteenth century revival referred to communal renewal of piety rather than specific occurrences of religious enthusiasm, and that such revivals met the need for renewal of the national covenant as the clergy wrestled with increasing secularization, loss of influence, and declining piety. Driven to action by specific incidents of revived religion, evangelicals in Britain and colonial America drew on elements already present in Reformed piety and theology in order to shape a model of "vital piety" and the reform of morals. Their expectations eventually found fulfillment in a widespread transatlantic revival. Crawford emphasizes several key moments in these developments, such as Jonathan Edwards's reflection on revival (which brought system to inchoate ideas and gave revivals a central place in the whole history of redemption leading to the millennium), George Whitefield's preaching, and the Cumbuslang revival in Scotland in 1742. The author is especially effective in calling attention to the extensive transatlantic communication among the leaders of the revival, demonstrating Edwards's central role in this, too. For Crawford, revivalism was as much a British as an American phenomenon. He also points to the importance in the revivals of a new kind of emotionally charged hymnody.

Crawford integrates his conclusions with a number of themes currently prominent in the study of the Great Awakening. He stresses, for example, its continuity with earlier piety, its Scottish background, and its character, in common with other revivals, as communal ritual. Crawford also features the role played by revival in maintaining clerical authority, as evangelical charisma filled the vacuum left by increasing secularization and the retreat of religious establishments.

Crawford has produced a fine synthesis of recent work in the study of the Great Awakening and revivalism. He evinces a deft touch in the handling of theological topics, avoiding reductionism while not neglecting the social context for theological developments (although it is inaccurate to consider high and hyper Calvinism as synonymous [p. 58]). There are, however, some unanswered questions. Was the idea of national covenant as important a factor in the emergence of revival as the author assumes? Crawford fails to prove this point, and for him to establish it firmly would entail better definition of national covenant and its relation to other covenantal concepts and more evidence of the pervasiveness of the notion than is provided. And if the Scottish influence was so significant, why is the Awakening in the middle colonies, where the Scots-Irish played so important a role, not only neglected but the region dismissed as a place into which revival had to be imported from elsewhere? Notwithstanding such queries, Crawford has written a stimulating and rewarding book in which much recent research, both his own and that of others, is ably synthesized.

Dewey D. Wallace, Jr.
George Washington University

Marcel Giraud. *A History of French Louisiana.* Volume 5, *The Company of the Indies, 1723–1731.* Translated by Brian Pearce. Baton Rouge: Louisiana State University Press. 1991. Pp. xii, 517. $47.50.

Volume five of Marcel Giraud's study of French Louisiana covers the eight years the Company of the Indies governed the colony. Published in French in 1987, it now mysteriously appears in English before volumes two, three, and four of the same work; volume one was published in English in 1974. A two-page preface summarizes the years between 1715 and 1723 (which the missing volumes cover), when Antoine Crozat and John Law sought to exploit Louisiana while the crown looked for revenue to offset its mammoth debt. Both men, however, failed to benefit either the government or the colony. Disillusioned, the crown assumed management of Louisiana for two years. In 1723 Law's Company of the West was restructured as the Company of the Indies, with its own directors general and freedom of management. But detrimental to the colony was its subservient position to the company's head office in France.

Giraud tells an engrossing story of the Company of the Indies' determination to profit, even at the ex-

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pense of the Louisiana colonists. It cut expenses to the bone, including defense, presents to Native Americans, and trade goods; raised prices for French imports and lowered them for Louisiana's few exports; and neglected the outlying regions of Mobile, Natchitoches, and Illinois. Moreover, the company's selection of incompetent administrators and their interrivalry severely weakened administration. The two religious orders in the colony were either inadequately funded (the Jesuits) or poorly suited to carry out their tasks (the Capuchins). Meanwhile, life in New Orleans was austere as shortages occurred in foodstuffs, clothing, and building materials and, in summers, the death rate soared. Despite the hardships, class-conscious individuals sought recognition as nobles, titles, and uniforms, and chafed when snubbed. The lack of discipline among the inhabitants was flagrant, and pointless lawsuits flourished. In 1725 and 1726, the disgruntled colonists rebelled against the company's policies that had reduced most of them to poverty. The crown, however, sided with its agents. Finally, the Natchez Indian rebellion (1729), induced by cruel and bungling military commandants, and the costs of crushing it compelled the company to relinquish the province.

Despite the company's injurious policies, Giraud notes that significant changes took place during these years. The search for precious metals was abandoned and the settlers in lower Louisiana turned gradually to agriculture, which made the colony more self-sufficient and less dependent on France. White immigration declined as substantial numbers of black slaves arrived, and they performed much of the arduous labor required to clear the land, farm, and build levees. Increasingly, colonists believed that only African slaves could ensure their economic survival. New Orleans also changed as more substantial structures replaced the hovels that were built originally.

Giraud does a commendable job in shedding light on this period in Louisiana's history, as he has done in the preceding volumes. His work is based virtually entirely on archival documentation; he makes little use of secondary works and cites almost none in his notes. His work is a welcome addition as it supplants the deficient nineteenth-century Louisiana histories that for too long have stood nearly alone in this time period. My only caveat is his inadequate discussion of relations with Native Americans. Unfortunately, volume five is Giraud's last and the history of the French Louisiana after 1731 must await a new author to finish the work he began forty years ago.

GILBERT C. DIN
EMERITUS
Fort Lewis College

ROBERT R. REA. *Major Robert Farmar of Mobile*. Tuscaloosa: University of Alabama Press. 1990. Pp. xii, 184. $31.95.

Robert Farmar was the kind of soldier with whom the British built an empire. But as a mid-level officer who was neither brilliant nor inept, he served in relative obscurity and has inspired little historical interest. Now Robert R. Rea has attempted to change that. Using civil and military records, Rea has written an account of the man and his career that he hopes "reflects the nature of his profession and its contributions to the making of America" (p. 3).

Unlike most of his peers, Farmar was born in America, the sixth son of a New Jersey planter-politician. He entered the military by raising a company of provincials for service during the War of Jenkins' Ear but soon obtained a captain's commission in the regular army and spent the rest of his military career there. He fought in the West Indies and the Low Countries during the War of Austrian Succession, endured thirteen years (1748–61) of garrison duty in Gibraltar and Scotland, and finally returned to combat at Havana in 1762. With the end of the Seven Years' War, Farmar assumed a position of greater visibility and his biography becomes much fuller. Half of the book is devoted to the period between 1763 and 1768, when Farmar supervised the transfer of Mobile to British control, established British authority in the Illinois Country, and fought with every civil or military official between Pensacola and Kaskaskia. Rea details both the charges and counter-charges that flew around Farmar during this time and the month-long court martial they precipitated. In the end Farmar was vindicated but chose to retire from the military and assume the role of a gentleman-planter outside Mobile. There he remained from 1770 until his death in 1778, building an estate and serving in the assembly of West Florida, events that inform Rea's final chapters.

Through this biography of Farmar, Rea hopes to accomplish two goals. The first is to show that Farmar was a figure who "links colonies old and new, America and the broader British empire of his time, provincial and imperial interests" (p. 1), and in one sense he succeeds. Farmar did move within the empire and thus established professional and personal links between its various parts. Rea fails, however, to carry that linkage much beyond Farmar's physical movements. He never discusses why Farmar might have joined or remained in the British army or even what Farmar thought of the empire he served for so long. His discussion of Farmar's response to the American Revolution, for example, is distressingly shallow. He quotes a letter from 1776 in which Farmar criticized "'the Harsh and coercive measures the British ministry and Parliament have thought proper to adopt'" in dealing with the colonies while noting in passing that two years later Farmar was one of sixteen prominent Mobilians to petition for more British troops to protect the region against American attacks (p. 140). Yet his only reference to this apparent change in Farmar's view is to acknowledge that "Major Robert Farmar, an American-born retired British army of-

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ficer, found himself caught in the center of the maelstrom of loyalties" (p. 140).

Rea's other aim is to reveal "the man within the uniform" (p. 1), and in this he has even less success. The reader learns nothing of Farmar's private life before 1764 and little after that. This is partially a result of the fact that Farmar left few personal papers. But part of the problem lies with Rea's failure to use what he has to its full potential. For example, he describes the furnishings and personal property detailed on the inventory of Farmar's estate but says little about what they suggest of the man's personality and values.

This is an excellent study of military administration in the first British empire and of the tremendous difficulty it involved (it was all the army could do to convene a simple court martial in Pensacola). It is not, however, an adequate account of Farmar the officer or the man.

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Virginia Polytechnic Institute and State University

James A. Huston. *Logistics of Liberty: American Services of Supply in the Revolutionary War and After*. Newark: University of Delaware Press. 1991. Pp. 373. $49.50.

James A. Huston, a chronicler of the operational and logistical aspects of modern warfare, has written a useful new survey. This highly readable study traces the young republic's attempts to sustain an effective army through its first five decades. Until recently, scholars shied away from probing the full logistical dimensions of America's early military establishment, even though the literature contained familiar accounts of Benedict Arnold's ill-equipped Quebec expedition and George Washington's hardships at Valley Forge. Attempts by researchers to examine the everyday frustrations of administering, supplying, quartering, and transporting the revolutionary armies proved challenging; such themes also attracted less attention than the heroic sagas of battles and campaigns. Louis C. Hatch only briefly dignified logistics in his pioneering study, *The Administration of the American Army* (1904), and he may have set a pattern; during the past decade, scholars such as Erna Risch and E. Wayne Carp have more extensively probed the logistics of revolutionary America. Huston himself published *The Sinews of War: Army Logistics, 1775–1953* a quarter century ago. The initial chapters of that survey provide the genesis of this book.

Huston excels when unraveling the administrative intricacies of Congress' quartermaster and commissary departments. The irregularities and corruption that undermined competitive bidding and overseas procurement practices led to reforms such as the contract system. The contributions of Quartermaster General Nathanael Greene and Finance Superintendent Robert Morris are acknowledged, but noteworthy officials such as Joseph Trumbull, Jeremiah Wadsworth, and Ephriam Blaine merit relatively scant attention. Even the author's assessment of Morris, whose wealth and influence were indispensable to military preparations for Yorktown, was apparently made without reference to any recent published monographs on the financier.

Huston skillfully includes several narrative accounts of punitive American expeditions against the Indian frontier, notable for their logistical inventiveness and swift movement, but which were strategically inconclusive. Marches led by George Rogers Clark and John Sullivan precipitated Indian reprisals of equal fury, counterproductive to American goals. Huston writes admiringly of Washington's efforts in the east, but he believes his talents "did not include a great deal of enterprise and ingenuity" in handling logistical arrangements (p. 198); too much paperwork and a rapid turnover in commissary officers impeded the commander in chief. The author portrays the Saratoga campaign, despite thorny interstate relations, to reveal the rebels' logistical system at its best, but he concedes that British defeat was equally due to John Burgoyne's incompetence. Huston strives so hard for balanced judgments that he often appears noncommittal.

In his final two chapters the author briefly, and unevenly, samples how postwar Americans struggled to establish their own domestic arms sources; rather predictably he focuses on contributions by Alexander Hamilton, Eli Whitney, and John C. Calhoun.

This book lacks a unifying conclusion, but its clear absorbing narrative is assisted by useful maps, illustrations, and an extensive bibliography. The result is a welcome overview of the crucial role logistics played in the early years of the republic.

John C. Cavanagh
Suffolk University

Richard Lowe. *Republicans and Reconstruction in Virginia, 1856–70*. Charlottesville: University Press of Virginia. 1991. Pp. vii, 261. $35.00.

In this work Richard Lowe presents a detailed account of Republicans' effort to secure control of Virginia during Reconstruction. In his introduction he challenges the "postrevisionist" interpretation of Reconstruction, which, he says, stresses the conservatism of Reconstruction, discerns only a limited Republican commitment to racial justice, and minimizes the accomplishments of the Republican Reconstruction program. Lowe argues that, on the contrary, the story of Virginia Republicanism documents Republican radicalism and commitment to racial justice and is a tale of significant accomplishments.

Lowe devotes only one chapter to describing the origins of Virginia Republicanism in the 1850s and the party's effort to maintain a loyal state government during the Civil War. In the following seven chapters he chronicles the Republicans' unsuccessful attempt

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to establish a party strong enough to retain power. Despite the challenge he poses to "postrevisionism" in his introduction, it is only in the final chapter that he draws on his chronicle to sustain his criticism.

Lowe provides the most thorough account presently available of Republican activities in Reconstruction Virginia. His description of Republican factionalism is particularly rich and detailed. Lowe confirms that Virginia reflected the pattern Michael Perman described in his *Road to Redemption* (1984): moderate Republicans, mostly native whites, urged a "competitive" appeal to white former Whigs and unionists; northern-born and black radicals urged an "expressive" appeal to energize Republican core voters, augmented by state constitutional provisions disfranchising part of the white electorate.

Lowe sympathizes with moderate Republican efforts to win white support. He argues that such an appeal could have succeeded and points to occasions where leading white conservatives indicated they would support a moderate Republican Party. The radical program could not work, Lowe insists, in a state with a large white majority of voters.

The most serious shortcoming of Lowe's study is his failure to describe how moderate and radical Republicans envisioned the future of Virginia. On what basis did moderates hope to secure white support? If merely by presenting an alternative to radicalism, Lowe's argument is not convincing. It was on exactly this basis that moderate conservatives and moderate Republicans cooperated in 1869. As Lowe himself chronicles, conservatives quickly jettisoned the moderate Republicans after the resulting victory because the radicals no longer presented a threat. To succeed, moderate Republicans had to have a program that would be more appealing to large numbers of white Virginians than the Conservative program. Lowe never tells us what that program was nor discusses the implications if it did not exist.

Likewise, Lowe does not attend to the radical vision. If Eric Foner is correct (*Reconstruction* [1988]), Republican Reconstruction established a regime of legal and administrative equality that enabled black laborers to negotiate freely over working conditions and compensation. Conciliating whites meant undermining this regime, and it is understandable that radicals decided to reject that alternative and opt for proscription instead.

These shortcomings render Lowe's book less insightful than it might have been. Nonetheless, he has written a useful work for scholars of Reconstruction, providing a firm basis of information to help assess the course of Reconstruction in the South.

MICHAEL LES BENEDICT
Ohio State University, Columbus

BROOKS D. SIMPSON. *Let Us Have Peace: Ulysses S. Grant and the Politics of War and Reconstruction, 1861–1868.* Chapel Hill: University of North Carolina Press. 1991. Pp. xx, 339. $34.95.

Most studies of U. S. Grant have dwelled either on his performance as general or as president, reinforcing the popular perception that he was unable to translate his remarkable military skills into effective political leadership. Brooks D. Simpson has challenged this dichotomy, contending that Grant intuitively understood Carl von Clausewitz's maxim that war and politics were inextricable, and hence sought to promote northern aims both by military and political means. To demonstrate this contention, Simpson analyzes Grant's political actions while he was a soldier, from the war's beginning to his election to the White House.

According to Simpson, for Grant both war making and peace making were to be directed toward reunification of the republic on a lasting basis. Initially Grant hoped this goal could be achieved by limiting the means and purpose of the war, so as to enhance reconciliation of southern whites when it ended. As the conflict unfolded, however, resolute Confederate resistance led him to endorse a war against the southern economy and civilian morale, as well as against its armies, and to accept the need for emancipation of slaves and their recruitment into the Union ranks. At the war's end, according to Simpson, continued evidence of southern white intransigence convinced Grant that the federal government would have to protect the rights of the freedmen and eventually enfranchise them as well.

Hence Grant became increasingly involved in the politics of Reconstruction, eventually siding with Congress in its struggle with President Andrew Johnson. Using his authority as commanding general, Grant encouraged Army officers in the South to limit the influence of unreconstructed ex-Confederates and to protect the interests of the freedmen. According to Simpson, Grant reluctantly decided to run for president in 1868 to ensure that the results of the war would not be overturned; this decision marked the final transformation of the general into the politician.

Much of Simpson's discussion of Grant's evolving views on Reconstruction, and his subsequent breach with Johnson, covers ground previously explored by Harold Hyman. He disputes Hyman's contention that Grant's actions were influenced by his desire for the presidency, and he makes a stronger case than Hyman, or Martin Mantell, who has also written about these matters, that Grant played a central role in the struggle over the control of Reconstruction policy. Simpson sharply disagrees with Grant's most recent biographer, William McFeely, who contends that Grant eagerly sought the presidency as an indication of the country's continuing esteem for him rather than because he had any agenda to implement. Simpson does not attempt to turn Grant into a Radical Republican, but he does demonstrate that Grant evinced a growing concern for guaranteeing equal

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rights, including suffrage, for blacks. Questions concerning Grant's desire for the presidency will not be settled by Simpson's account. But his careful review of Grant's evolving views on slavery and on the subsequent condition of the freedmen provides a valuable corrective to McFeely, who gives these topics little attention. This is a welcome addition to the literature on Grant.

RICHARD H. ABBOTT
Eastern Michigan University

PIA G. CELOZZI BALDELLI. *Arbitrati e politica di potenza: Gli Stati Uniti dopo la guerra di secessione*. Rome: La sapienza editrice. 1990. Pp. x, 389.

In 1871–72, having just passed through the difficult process of unification, Italy found itself at the center of the controversy between the United States and Great Britain over the Alabama Claims. The phrase served as the rubric for a list of American grievances highlighted by a demand for reparations for damage done to American shipping during the Civil War by confederate privateers outfitted in England. Pia G. Celozzi Baldelli tells this story from a European perspective, with an emphasis on the crucial Italian role in the settlement.

American historians have generally treated these events in the context of American Reconstruction-era politics and public opinion. But the controversy over the Alabama Claims followed hard on the heels of another major conflict with worldwide implications, the Franco-Prussian War. Consequently, a bilateral dispute appearing to have little significance beyond the interests of the two powers directly involved became intricately linked to the rise of German, Russian, and Italian nationalism, and the collapse of France—the creation, in other words, of a new European order.

Celozzi Baldelli tells this story well. The book reflects extensive work in archival and manuscript collections in this country, England, and Italy. Although her treatment of the American side of the controversy yields nothing new, she stresses the importance of the American success in the claims process on the evolution of an American national identity, the emergence of the country as a world power, and the recognition of that fact by the English. Happily, the book also pays considerable attention to the effects on Canada, then engaged in establishing its autonomy from England as well as defining its own national identity by distancing itself culturally and politically from the United States.

What is particularly good about this book is its emphasis on the effects of the Alabama Claims settlement process on Europe after the Franco-Prussian War, and particularly on Italy. On the ruins of the European balance of power that had endured since Waterloo, a new equilibrium was emerging. The author treats the impartial arbitration commission agreed to by the United States and Great Britain as a significant departure from the old European "politics of the congresses" (p. 356). She praises the United States and England for their initiation of what many hoped was to be a new era characterized by the voluntary settlement of disputes by international arbitration tribunals. She places these efforts in the context of other attempts at European cooperation and conciliation emerging at the same time and shows how the successful conclusion of the Alabama dispute invigorated those efforts, raising hopes among pacifist and internationalist circles for a future in which, according to Italian foreign minister Visconti Venosta, "peaceful judicial proceedings would be substituted for armed conflict" (p. 358).

For the new nation of Italy, the opportunity to play the pivotal role on the arbitration commission was of particular importance, not only in terms of its own national identity and international stature but also because of the effect on the Italian domestic political equilibrium and on the delicate question of international acceptance of the move of the Italian capital to Rome, home of the Papacy.

Of course, the hopes raised by the peaceful settlement of the Alabama Claims were soon dashed by the imperial ambitions that marked the end of the century and eventually resulted in two world wars. Nevertheless, the success of the impartial tribunal was a demonstration of the possibilities of international arbitration and heralded as such at the time. Perhaps more significantly, the settlement had profound implications for the history of the world because it initiated the unshakable Anglo-American alliance that proved to be the most significant and enduring geopolitical factor of the twentieth century. Celozzi Baldelli's book adds significantly to our understanding of the international ramifications of the American Civil War.

RONALD L. FILIPPELLI
Pennsylvania State University

WILLIAM COHEN. *At Freedom's Edge: Black Mobility and the Southern White Quest for Racial Control, 1861–1915*. Baton Rouge: Louisiana State University Press. 1991. Pp. xix, 340. Cloth $42.50, paper $16.95.

The major contribution in William Cohen's study is the analysis of southern blacks' legal status as nominally free laborers from 1865 to 1915, a topic that he masterfully treated in his article "Negro Involuntary Servitude in the South, 1865–1940: A Preliminary Analysis" (*Journal of Southern History* 42 [1976], 31–60). This book places the problem of freed blacks' status in a broader framework by examining two important features of race relations magnified by the emancipation of slaves: geographic mobility among the freedmen and whites' efforts to control free black labor. The relationship between these two parts of Cohen's analysis remains problematic, however.

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Cohen describes a postwar labor system in which black mobility was impeded in the interests of landowners. He traces the origins of this system from the Black Laws of 1865–66 to the edicts of the Freedman's Bureau, and on to the statutes and court decisions of Redeemer state governments. Cohen thoroughly delineates legal questions concerning enticement of labor, vagrancy, and the creditor-debtor relations that suffused southern agriculture after the Civil War. Although little of this information is fresh historical material, Cohen provides an expert guide to the complexities of rural labor relations between white landowners/creditors and black laborers/debtors.

On the other side of his proposed equation, Cohen pays particular attention to the volume and direction of blacks' geographic movement and to the auspices under which freedmen and their children moved. The Back-to-Africa campaigns of the late nineteenth century and blacks' efforts to establish communities in unsettled new lands in Kansas and Oklahoma are reviewed, as is black migration within the South and between the South and the North. By bringing together these population movements in one narrative, Cohen convinces us that black migration had reached significant levels well before it received widespread public notice and comment. More importantly, his examination of the little-known roles of both black and white labor agents in these episodes illuminates some aspects of the mechanics and modes of organization behind the migrations.

This book's explorations in the meanings of geographic mobility for blacks' free-labor status are less successful. The available quantitative data severely limit precise analysis and firm conclusions about migration in the post–Civil War era. Despite admirable use of available newspapers, government records, and manuscript sources, Cohen lacks documents expressing southern blacks' own views that would show how the freed slaves understood the conflicting possibilities and constraints in their lives.

Cohen's study contributes to our understanding of two different phenomena but does not successfully connect them within his concept of a post–Civil War labor system. He recognizes that whites' efforts to control black labor were neither systematic nor monolithic; some employers encouraged sharecroppers and laborers to leave their lands while at the same time others resorted to laws and extra-legal means to immobilize black labor. His analysis of black migration nevertheless does not portray it simply as a reaction against labor controls. Given the poverty of rural southern blacks in the late nineteenth and early twentieth centuries, most of them would probably have moved to new places in the hope of improving their condition, regardless of southern whites' labor policies. The long antecedents of both whites' labor control and black mobility in the South make it difficult to posit any direct or simple relationship between them.

PETER GOTTLIEB
State Historical Society of Wisconsin
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GARY R. KREMER. *James Milton Turner and the Promise of America.* (Missouri Biography Series.) Columbia: University of Missouri Press. 1991. Pp. xii, 245. $32.50.

James Milton Turner played a small but illuminating role in the unfolding of "freedom" for African Americans after emancipation. He, like most black men and women, genuinely expected that the end of slavery and the onset of Reconstruction would bring a full range of opportunities for citizenship. More personally, however, Turner expected that his superior education and his staunch support for the Republican Party would pave the way for leadership, public recognition, and material reward.

Turner's principal public arena was Missouri, especially St. Louis, but his interests and expectations transcended the opportunities and developments in that southern state. Freed from slavery as a young child, he had the unusual benefits of education, including a brief time at Oberlin College in the 1850s. He also developed a strong sense of confidence, even arrogance, and a virtually unsurpassed gift for oratory. By stepping quickly onto the emerging but shifting political stage in 1865, Turner anticipated becoming a leader of his race and a major contributor to the expansion of opportunities for African Americans who were willing to invest hard work, patience, and education in the future of America.

He was doomed to disappointment. Gary R. Kremer presents a heavily researched and sometimes rather tedious account of Turner's public life. Each chapter is a new episode: his role in the campaign for a system of public education, his attempts to win political election or appointment to office, his "high tide" as minister resident and consul general to Liberia from 1871 until 1878, his involvement in several schemes to aid black emigrants to Kansas, his efforts on behalf of himself and the Cherokee freedmen in the Indian Territory, and his rather pitiful campaign late in his life to win the honor and status he long coveted from Missouri Prince Hall Masons.

Most, if not all, of these efforts contributed to a sense of disillusionment and rejection. Turner kept coming back, head raised, golden oratory ringing forth to any audience he could command or invent. He bounced from issue to issue, Republican Party to Democrat, steadily losing credibility and influence among African Americans as well as whites. Kremer sees this personal tragedy primarily as the result of Turner's clash with the strictures that white racism placed on black participation in the promise and

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progress of America. Certainly this must be true, but the story Kremer presents is also an account of an immensely ambitious and egotistical man who rarely saw himself or his times clearly.

LESTER C. LAMON
Indiana University, South Bend

WILSON JEREMIAH MOSES. *Alexander Crummell: A Study of Civilization and Discontent.* New York: Oxford University Press. 1989. Pp. viii, 380.

Alexander Crummell was widely known and respected among his nineteenth-century contemporaries but became an obscure figure for later generations. Wilson Jeremiah Moses laments the neglect of Crummell, because in him "one can identify the sources of both black nationalism and black conservatism," two mentalities that "are not necessarily very far apart" (p. 9). This book not only provides significant insight into African-American ideological developments but also resurrects a fascinating human being.

Crummell was born in 1819 in New York to a free-born black woman and a kidnapped African who unilaterally declared his freedom after serving his master ten years. Alexander frequently boasted that both his parents were of "full-blooded" African descent. As a child, he was educated with the black elite at the African Free School and later at Oneida Institute. Through the influence of Peter Williams, Crummell declared his intention to become an Episcopal priest in 1838. This began a lifelong, bruising battle with the Episcopal hierarchy for recognition and respect. Crummell lost the first round; he was not admitted to the Episcopal seminary.

After studying unofficially at Yale, Crummell was ordained in 1844. To varying degrees he failed to win the support of his parishioners in Providence, Philadelphia, and New York before leaving in 1848 for England. There he solicited funds both for his church and his own education. When he left in 1853, he had earned a degree from Cambridge. Instead of returning to New York, however, he became a missionary to Liberia. For about sixteen of the next twenty years he labored there, walking "a tightrope between African nationalism and his loyalty to the Episcopal Church" (p. 90). A coup forced him to flee in 1872.

Crummell settled in Washington, D.C., serving as an Episcopal priest until his retirement and his founding of the elitist American Negro Academy in 1897, two years before his death. Rigid and puritanical, he was largely a failure as a husband and father and was at odds with several contemporary black leaders. Nevertheless, he profoundly influenced W. E. B. DuBois and several men who became prominent disciples of Marcus Garvey.

In a brilliant final chapter, Moses compares Crummell with other African-American leaders. As the entire book reflects, the ideology of black leadership is extremely complex, and all leaders are guilty of apparent contradictions. For example, Moses contrasts Crummell's desires for political independence with his call for cultural assimilation, as well as Crummell's desires for ecclesiastical independence yet "temporary economic dependency" (p. 295). Perhaps some explanation is found by describing the philosophy of black leadership as based on several independent continua: accommodation to militancy; separatism to integration; self-help to demands for reparations; cultural nationalism to assimilation; and individualism to collectivism. Most black leaders locate (and sometimes relocate) themselves along each continuum for a variety of personal as well as philosophical reasons. They are also shaped by secular and theological ideas not directly related to the black experience.

Thus, Crummell's conservative beliefs about "civilization" and "progress" influenced his programs of black uplift—as did his personality conflicts with other leaders. By accepting that civilization was an absolute, universal state to be discovered rather than created, Crummell combined militancy and separatism with cultural assimilation. This led to "perpetual discontent" because he "adjusted far too well to the values of 'civilization.'" And as Moses notes, Crummell's life shows that white prejudices were "based on color, not culture" and were undimmed by black "ability to understand Greek syntax or the binomial theorem" (p. 275).

LINDA O. MCMURRY
North Carolina State University

CARL J. GUARNERI. *The Utopian Alternative: Fourierism in Nineteenth-Century America.* Ithaca: Cornell University Press. 1991. Pp. xiv, 525. $32.95.

Over fifty years ago Arthur Bestor, Jr., completed a pioneering dissertation at Yale titled "American Phalanxes: A Study of Fourierist Socialism in the United States (with Special Reference to the Movement in Western New York)." That unpublished work has remained, until now, the authoritative work on the topic. This new interpretation by Carl J. Guarneri surpasses it because of its wider scope, the range of materials used, and a framework that is more varied and complex.

Guarneri examines Fourierism's French origins, the early "apostles of association" (four from New York, eight from New England) who kept it in the forefront of public and intellectual opinion, the sources of membership, the doctrine (modified by Albert Brisbane and others), and the Fourierist movement from 1832 to 1846. In a final section he looks at its decline in the 1850s, its influence on labor associations, and its legacy in the 1870s and 1880s toward new communal efforts. At its peak it enlisted some of the best minds and moral critics of its generation

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(Horace Greeley, Parke Godwin, George Ripley, Charles Dana, William Channing, Timothy Dwight, Henry B. Blackwell) and an estimated 100,000 supporters.

Brisbane not only interpreted and simplified Fourier for an American audience but also moderated its potentially explosive sexual doctrine of "attractions" for an evangelical audience. Beyond Brisbane's bowdlerization Guarneri discusses the essential plasticity of Fourier's ideas that allowed workers to see its cooperative features as a promise of economic security and an alternative to wage labor, allowed philanthropists to see it as a "scientific" system promoting social harmony, and allowed reformers to see it as a radical venture in self-expression. Fourierism was middle class in its origins and leadership, concerned with social order and organization, but still attractive to artisans who formed the majority of the phalanx members.

There were distinct differences between the leadership in France (theoretical and haughty) and in the United States (practical and moral) and between New Yorkers (theorists) and New Englanders (practical minded). Brook Farm was the center of this movement, and here Guarneri gives a detailed picture of life within the phalanxes that modified Fourierist theory because practical considerations dictated adjustments. For example, skilled laborers sometimes objected to being shifted around and sometimes remained at the same jobs because marketable goods had to be produced by skilled artisans. The movement collapsed in 1846–47 but lived on in vital fashion as part of workers' associations, Protective Union Cooperatives, and in labor ideology. Simply put, Fourierism's decline can be attributed to prosperity, to the prominence of other national issues for reformers (slavery and expansion), and to press attacks on the movement as antireligious.

Never once does Guarneri condescend to see them as quaint eccentrics, and he provides detailed and useful appendices to show membership characteristics, shareholding percentages, and patterns of affiliation. It is best compared with John F. C. Harrison's study of Owenites in Britain and America (*Quest for a New Moral World* [1969]) in that both authors see their movements as part of a whole and as responding to national issues. These Fourierites were more compelling than, say, the Workingmen's Party, and Guarneri portrays them as practical-minded American utopians despite their failure to conceive slavery as a political issue or to alter women's roles in their communities. They were, to some degree, conservative. But they did attack destructive competition, force an examination of the "marriage question" in some circles, and lend a supportive voice to labor.

Guarneri has written the most comprehensive and sensitive account of this movement that we have, placing it at the center of issues rather than marginalizing it. This work should stand as a scholarly landmark for another half-century.

ROBERT S. FOGARTY
Antioch College

THEODORE STEINBERG. *Nature Incorporated: Industrialization and the Waters of New England.* (Studies in Environment and History.) New York: Cambridge University Press. 1991. Pp. xv, 284. $34.50.

Scholarly concern with ecological history has been an exciting development of recent years. Donald Worster, William Cronon, Carolyn Merchant, and others have shaped our understanding of the American environment, particularly with respect to agriculture. Theodore Steinberg, in a lucid, useful case study now extends this inquiry to the early consequences of industrialization.

He focuses on the efforts of the Boston Associates to control the waters of the Charles and Merrimack rivers to provide power for the textile mills of the Waltham-Lowell system. Steinberg shows that alongside the now-familiar story of these mills, their commercial and technical innovation, urban growth, and labor struggles, lay another struggle to redirect nature for productive ends. Industrial capitalism, he argues, entailed new sets of ecological as well as social relationships. Manufacturers' bold efforts to reshape New England riverscapes helped cement the instrumental conception of nature as a resource to be dominated.

Steinberg traces the systematic use of rivers to power machinery, the construction of dams and canals, and the use of corporations to "sell" water once common property. Not primarily a technical or engineering history, the book focuses on the changed human relationship with nature that this exploitation entailed, and particularly on the legal history of the Boston Associates' dealings with other users of rivers and riparian land. While drawing on the work of Morton Horwitz and others, Steinberg adds to our understanding of the evolution of legal principles that favored economic development.

Production demanded regular water supply, and the book's second half contains a fascinating account of the Boston Associates' efforts—using a local corporation as a front—to control the New Hampshire lakes that fed the Merrimack River. Farmers, loggers, fishing companies, and others sued them, attacked their dams, or petitioned state authorities to curb their powers, and the decisions in these cases, often favorable to the Associates, embodied the instrumental attitude to nature that is Steinberg's central theme. But the notion that resources should be made amenable to economic use was not, he suggests, merely a product of the judicial mind. In the 1860s and 1870s, attempts to replenish the Merrimack's fish stocks included the use of artificial propagation. Early public health experts' efforts to control disease included

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water and sewage networks developed on the model of the manufacturers' systems of power supply. Each case, to which Steinberg devotes separate chapters, extended the notion that natural phenomena could be subjected to human control, a notion that many of the Boston Associates reflected not just in their exploitation of the rivers but also in an interest in horticulture. Even the men who sought damages and attacked dams, Steinberg argues, accepted the idea that resources were to be exploited; their argument was merely over how the benefits should be distributed.

Steinberg suggests there was a consensus on this fundamental—in his own view, flawed—conception of nature, shared (it seems) by everyone but Henry David Thoreau. His fine book is a challenge to other scholars to verify or, alternatively, to modify this contention.

CHRISTOPHER CLARK
University of York

WILLIAM CRONON. *Nature's Metropolis: Chicago and the Great West*. New York: W. W. Norton. 1991. Pp. xxiii, 530. $22.95.

In this work, William Cronon continues a scholarly career dedicated to demonstrating what history can learn from ecology. Cronon's first major book, the celebrated *Changes in the Land: Indians, Colonists, and the Ecology of New England* (1983), examined environmental modification immediately before and after the initial contact of European settlers with the northeastern coast. In that book, and in the present volume, Cronon points out that what we call "nature" is a complex mosaic of original and constructed, people-caused conditions. Obviously, original or, in Cronon's terms, "first nature" (p. 264) determined the pre-human environment. But thereafter, the most powerful force shaping the ecosystem derived from the ambition and ingenuity of people. Cronon's goal for environmental history is very close to that of ecology: understanding the interrelationships between humans and the natural world.

Cronon shifts his focus in this book several centuries later and several thousand miles westward from colonial New England. His narrative revolves around the city of Chicago, but his thesis is that neither this metropolis nor any city can be understood apart from its environmental and economic hinterland. In the case of Chicago, the region was nothing less than a huge slice of North America extending from the Great Lakes to the Rocky Mountains—the "Great West."

Cronon takes pains to "tell the city-country story as a unified narrative" (p. xiv). Ecology-like, he integrates rather than separates. He emphasizes constantly that urban and rural areas are parts of an interconnected landscape and share an interconnected history. The "environment," Cronon argues, is not just nature. Environmental historians must study urban and economic developments as well. History, like ecology, should strive for seamlessness. So, Cronon writes, "The history of the Great West is a long dialogue between the place we call the city and the place we call the country" (p. 54).

Today, as the centennial of his controversial essay on the American frontier approaches in 1993, Frederick Jackson Turner has apparently become the whipping boy of every Western historian. Cronon is gracious about it, but he follows suit. His principal complaint is that Turner persuaded several generations of Americans that the frontier, way out there, had nothing to do with the urban civilization thousands of miles to the east. The frontier was the new world, and by the time cities appeared it had vanished. Cronon does not see it this way. The frontier, or as he calls it "the country," is linked commercially and, in a real sense, ecologically to "the city." For Turner, in other words, the isolation of the frontier explained American development. For Cronon the frontier was never isolated. The West was not a wilderness but part of an urban empire.

Cronon sweeps from the 1830s, when Chicago (the place of wild garlic) took shape as a white community, to 1893, when the city on the big lake hosted the World's Fair (at which Turner delivered his famous frontier address). As might be expected in this kind of integrative book, Cronon writes about a wide range of subjects. Most of them have been treated in more detail by others, but Cronon's forte is synthesis. We learn in his book about railroads, reapers, refrigerated meat cars, grain elevators, credit and bankruptcy networks, and futures markets. Three chapters are organized around specific resources: grain, lumber, and beef. In each case Cronon shows how the chains of causation that altered—and he is frank to point out, devastated—some environments, extend from the frontier through Chicago to Eastern European markets. The buffalo gave way to cows, the native prairie grasses to wheat, and the majestic white pine to the desolate Cutover Lands. Cronon is sensitive to the liabilities as well as the benefits of development. He makes clear that environmental responsibility must be premised on recognition of, as he puts it, "the ecological consequences of our own lives" (p. xv). It follows that even as residents of intensely urban areas, like Chicago, we must accept responsibility for what happens in ecosystems far away. Cronon is to be applauded for writing a book that is both scholarly and relevant to understanding and solving serious contemporary environmental problems.

RODERICK FRAZIER NASH
University of California, Santa Barbara

C. ROBERT HAYWOOD. *Victorian West: Class and Culture in Kansas Cattle Towns*. Lawrence: University Press of Kansas. 1991. Pp. xiv, 325.

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The story of the Kansas cattle towns is one that seemingly has been done to death by both scholarly writers and the mythmakers of the popular culture machine. Yet C. Robert Haywood has found something fresh and interesting to say about three of those towns, Dodge City, Wichita, and Caldwell. His book describes the other side of cattle town life, the portion that venerated respectability rather than high jinks, "civilization" over "roistering." His point is that the cattle towns were ultimately of a piece with the broader American culture that prevailed in more settled areas. The mentality that prevailed in cattle towns, admittedly after some struggle, was Victorianism, that conglomeration of middle-class beliefs about respectability, order, duty, style, "manners and family and class roles" (p. 4). Certainly Wichita was not Boston, but the similarities of culture and class distinctions were marked.

To illustrate the ties and continuities between old towns and cattle towns Haywood first describes the class structure and rank within the Kansas communities. In the United States, class has never been about money alone. People of color in the cattle towns tended to be a group apart, no matter their income, while wealthy white doctors or lawyers were of the elite, but only if their behavior toward family or alcohol consumption were above reproach. After delineating the distinctions among classes and individuals, Haywood explains how the "civilized" elements lived, the institutions they created, and the differences between their behavior and that of the rougher crowd. Marriage and its attendant ceremonies provides an interesting example. Class played an important role in the style of the ceremony and the newspaper coverage of it. The civilized element held ceremonies of dignity and decorum; the paper reported speeches about progress and stability. Coverage of a ceremony among the rougher element recounted with amusement the drunken state of the groom. It was clear with which couples the future of the town lay.

This book is rich in detail about daily life, social organizations such as lodges, institutions such as the school and the church, as well as beliefs about death, attitudes about women's roles, Victorian hopes for the future, and other social ideals. Again and again Haywood returns to his theme: the respectable folk of the cattle towns shared the national Victorian culture and eventually imposed it fully on their communities.

The volume is nicely designed with a variety of pictures and illustrations. The text includes samples of contemporary poems, songs, social reports from the newspapers, and a variety of other descriptive materials, including advertisements, that convey the qualities of uplift and respectability so important to the middle classes in the late nineteenth century. Haywood also uses effectively a large number of secondary works from several disciplines to provide a framework for his original research. His overuse of coy or clever nineteenth-century phrases such as "soiled dove" or "fallen sisters" for prostitutes is the only discordant note in an excellent work on culture in a place in transition at a time of transition for the United States.

PAULA M. NELSON
University of Wisconsin, Platteville

RICHARD FRANKLIN BENSEL. *Yankee Leviathan: The Origins of Central State Authority in America, 1859–1877*. New York: Cambridge University Press. Pp. xi, 452. Cloth $59.50, paper $18.95.

"The American state emerged from the wreckage of the Civil War," argues Richard Franklin Bensel, and here he provides readers with "an account of American state formation" (p. ix), between 1859 and 1877. During these years "an extremely weak antebellum state" assumed "the fundamental attributes of territorial and governmental sovereignty" (pp. 1–2) but the results, Bensel believes, reflected the impact of southern separatism in ways that negate either a class-conflict or a consensus interpretation of American history. This book synthesizes a substantial secondary literature and fresh legislative analysis within a conceptual framework derived from the field of political economy.

After critically discussing modernization theories and the American case, Bensel constructs the political economy of secession and the American Civil War. Why did the South secede and why did the North resist its departure? He suggests that the threat of the Republican social and economic agenda made secession a rational decision; northerners resisted because states have typically resisted peripheral erosion and because northern leaders understood that further subdivision might occur if the southerners succeeded. There follows an account of war mobilization within the two states in which Bensel compares their efforts on forty-two dimensions of structural and operational strength. Although the North won the war because of superior resources, southerners moved even further toward statism. Northern leaders retained the premodern system of political patronage and effected mobilization through a resort to contractual arrangements within the private sector. A new class of financial capitalists emerged in the North while another—the slaveholders—was being destroyed.

Bensel describes the financial system in its war and postwar aspects, explaining the various interests involved and the growing dissatisfaction of finance capitalists with the Republican Party's policies during Reconstruction. He identifies four major types of congressional constituencies: those with well-developed national banking facilities, iron and steel districts, southern and western electoral units (still undergoing settlement), and those in the border and former Confederate states. Interaction of these interests at the congressional level set the dimensions of

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the political economy of the Reconstruction era. Soon of secondary interest in this interplay, the effort to reconstruct the South through external statist policies withered and the progress of modernization within the American state decelerated. Bensel believes it ironic that further progress down that road and the development of formidable state regulatory powers owed much to southern congressmen. Other implications of the southern presence included a redistribution of resources at southern expense, which dampened western political disenchantment within the Republican Party temporarily, and a "cross-sectional inversion of class alignments of the two major parties [that] made the emergence of coherent lower class claims difficult, if not impossible, in the national political arena" (p. 421).

Bensel provides much factual substance and his argument is sometimes complex; a short review cannot do full justice to his interesting contentions. It is heartening, however, that this author has developed so thoughtful a treatment of the economic foundations of politics in so important a period. Some of his emphases, methodology, and conclusions may be challenged. But Bensel's book is a useful corrective to recent interpretations of the Civil War era and one that anyone interested in this crucial era must read.

ALLAN G. BOGUE
University of Wisconsin, Madison

SARAH LYONS WATTS. *Order against Chaos: Business Culture and Labor Ideology in America, 1880–1915.* (Contributions in Labor Studies, number 32.) New York: Greenwood. 1991. Pp. xii, 196. $45.00.

A perennial question in the historiography of the United States is why socialism has enjoyed such little success here. One answer points to the weaknesses of the American Left and the conservatism of the working class. Sarah Lyons Watts emphasizes instead the ferocity and effectiveness of capital's ideological struggle against those who would challenge its dominion, notably the socialists, unionists, and Populists who questioned the emergent industrial order at the end of the nineteenth century. In her view, business leaders in the years between 1880 and 1915 formulated and popularized a labor ideology that "transformed the republican ideal of labor into praise of the social contributions of business and condemnation of workers' disruption of the social order" (p. 4). Their new ideology focused not on the dignity of free labor but rather on the transcendent merit of efficiency, scientific technology, and individualism. In the end, the triumph of a business vision of labor's proper role in modern society worked to block the development of anticapitalist impulses, and thus contributed significantly to the profoundly conservative nature of modern America.

Watts builds her argument around three case studies. The first examines the Pullman strike, where business leaders worked to identify the interests of large capital with those of the population as a whole. The second examines the formulation and implementation of modern management techniques, which, in the hands of business leaders and experts such as Frederick Taylor and Hugo Munsterberg, enshrined scientifically increased productivity as the supreme goal of both workers and managers. The third details the educational and political activities of the National Association of Manufacturers in the years after 1900. The case studies cover familiar ground and yield no surprises.

What is original is Watts's use of the concept of cultural hegemony to unify her material and establish her interpretive theme. The business activities Watts describes constituted "cultural warfare" (p. 38). The impact of business' labor ideology derived from the fact that the assumptions, language, definitions, and conclusions of the new creed came quickly to constitute a new economic and intellectual reality. In turn, that reality determined what constituted responsible discourse on labor issues, and indeed what was an issue and what was not.

The author's exploration of cultural hegemony highlights both that theoretical framework's possibilities and its problems. The concept enables her to see with new clarity the relatedness of seemingly disparate events and developments. On this score Watts's account is compelling. In other regards, however, the concept remains both slippery and troubling. It lends itself perhaps too readily to a kind of ideological special pleading. "Middle-class definitions of the good and proper society" became "ideologies of class domination" (p. 38), never achieving the authenticity and legitimacy that Watts grants as a matter of course to working-class values and practices. The author appears to use the adjectives "middle class" and "bourgeois" not only as descriptive labels but as epithets as well, and one wonders whether the study does not move too easily from condemning the imposition of middle-class values to condemning the values themselves. Still, the concept of cultural hegemony has considerable explanatory power in the American context, and other scholars are sure to follow the somewhat problematic lead provided by this study.

ROBERT M. COLLINS
University of Missouri, Columbia

CLARK D. HALKER. *For Democracy, Workers, and God: Labor Song-Poems and Labor Protest, 1865–95.* (The Working Class in American History.) Urbana: University of Illinois Press. 1991. Pp. xiii, 243. $29.95.

Through the peephole of a single form of popular expression, Clark D. Halker leads his readers on a most enjoyable review of a robust period of American labor protest. His vehicle is the labor song-poem that

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regularly adorned the pages of the nation's far-flung labor press in the post–Civil War years. Based on a reading of nearly 2,600 documents drawn from forty-three English-language publications, Halker examines the authors, form, and content of the poems, all the while attempting to account for their rapid emergence and ultimate decline in terms of the larger political and cultural context surrounding the labor movement.

Generally, Halker's scribbling troubadours neatly fit—both in personal profile and in their messages—the recent picture drawn of the Gilded Age labor movement. A cross-class collection of authors weighted toward skilled white males who combined millenarian expectations of a more egalitarian social order with a labor-republican twist on traditional cultural values and fiercely nationalistic loyalties effectively defines the genre. Sources of inspiration were widespread and changed over time. While "Puddler-Poet" Michael McGovern and railroad bard "Shandy Maguire" often drew on Irish folk ballads, Civil War songs (especially "John Brown's Body" and "Tramp, Tramp, Tramp"), the minstrel stage, and ultimately Tin Pan Alley also furnished original material from which labor derivationists drew. Halker further stresses a link between labor culture and the still-underexplored antebellum spiritualist movement as the first social movement to use song-poetry as a vital element within its ranks. Over a quarter of all borrowed labor tunes, Halker reports, came from religious hymns, including the two most famous expressions of the period, the Knights of Labor adaptation of the evangelical hymn, "Hold the Fort," and I. G. Blanchard and Rev. Jesse H. Jones's "Eight Hours."

Analytically, perhaps Halker's major contributions lie both in his renewed emphasis on Christian influence within the labor movement and in his shrewd characterization of the eclipse of the autonomous culture of the song-poem by the rise of more commercially produced, popular entertainment. Although the labor poets willingly savaged every convenient foe, Halker's account suggests that they saved their most precious venom for capitalistic greed outfitted in Sunday Christianity. Thus, one contributor to *Fincher's Trades Review* in 1864 snarled, "Oh, hypocrites, hypocrites, what will ye say / When you stand before God on that awful day / When the earth like vapor shall pass away— / And God's children shall call upon Him to repay / All the wrongs you have heaped upon them?" (p. 153), while Arthur Cheesewright in 1887 offered a more personal insult in the Denver *Labor Enquirer*: "Here lie the bones of Henry Ward Beecher, / A Foe to mankind and an overfed preacher; / Good enough for the poor was water and bread; / His life it is o'er—Thank God he is dead" (p. 151).

Finally, Halker deserves a nomination for the best acknowledgement in recent memory. Taking a cue from the eloquence of his historical subjects, the author refers to his "best critic and editor" who "taught me the value of a single kiss and a compact sentence. In an age of atomic chance, I'd still rather bet on romance" (p. xiii).

LEON FINK
University of North Carolina, Chapel Hill

GARY Y. OKIHIRO. *Cane Fires: The Anti-Japanese Movement in Hawaii, 1865–1945*. (Asian American History and Culture Series.) Philadelphia: Temple University Press. 1991. Pp. xvii, 330. $39.95.

It has been Hawaii's happy fate to be treated as the exception in the historiography of anti-Asianism in the United States. With the Japanese in particular, the assumption has been that those who lived in the Hawaiian Islands experienced little if any of the organized racism encountered on the mainland. Most notably, they managed to escape the concentration camps and other outrages committed during World War II against Japanese on the West Coast, where they made up less than 2 percent of the population as opposed to 37 percent in Hawaii.

That is not how Gary Y. Okihiro interprets the Japanese experience in Hawaii. He contends that from the time they first arrived there in 1865, the Japanese became victims of an overlapping racist-capitalist-class-bureaucratic conspiracy similar in spirit if not scale to what they faced on the mainland. Most sinister of the many forces Okihiro identifies as working to deny Japanese the American dream was the island's own caucasian plutocracy. This cunning elite rose from obscurity in the nineteenth century to control Hawaii's lucrative sugar industry and with it, after annexation to the United States in 1900, the territorial government as well. Their policy toward the Japanese remained constant up to World War II. By increasingly deceitful means, they sought to keep the Japanese as a cheap, captive labor force on the sugar plantations. Inevitably, labor relations in Hawaii became inseparable from race relations until finally, after the plantation strike of 1920, management took the initiative in fomenting an anti-Japanese movement.

How the sugar industry and its allies went about manipulating both public opinion and policy in the 1920s and 1930s to turn the Japanese into a permanent underclass is the most valuable part of Okihiro's study. The strategy hinged on persuading federal authorities that the Japanese had to be kept repressed because they could not be trusted. American-born or not, the claim went, the Japanese were loyal to Japan, not the United States. The scheme led to a series of contingency plans in which various military authorities and the FBI, with President Franklin Roosevelt's strong encouragement, defined the preemptive measures to be taken against Hawaii's Japanese community in the event of war between Japan and the United States. The mass arrests and other abuses

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suffered by the Japanese in Hawaii following the attack on Pearl Harbor were therefore not spontaneous events, as has generally been assumed, but part of a master plan approved beforehand at the highest levels. By Okihiro's count, 1,466 local Japanese spent the war years in concentration camps in Hawaii, while another 1,875 were shipped to camps on the mainland.

Not all readers will find Okihiro's account persuasive. Some will be less impressed by the originality of his research than the contentious, Japanese-always-sinned-against-and-never-sinning nature of his narrative. His description of events prior to 1920 is especially open to challenge, for only infrequently does it reflect research in the extensive primary documentation now available. He is to be congratulated, nevertheless, for opening up a fresh perspective on race relations in prewar Hawaii, however predisposed his judgments about the treatment of the Japanese turn out to be.

ROBERT J. GOWEN
East Carolina University

LESTER S. KING. *Transformations in American Medicine: From Benjamin Rush to William Osler*. Baltimore: Johns Hopkins University Press. 1991. Pp. 268. $38.00.

In this interesting and important book, Lester S. King focuses on medical theory rather than practice and describes how the American medical world of the eighteenth century was transformed into the American medical world of the nineteenth century. With an emphasis on changing modes of thinking, and on the changing approaches to fever, King provides detailed information on the ideas and works of many European and American physicians and scientists, including Elisha Bartlett, François Broussais, Charles Caldwell, William Cullen, John Eberle, Austin Flint, William Osler, Benjamin Rush, Nathan Smith, and Thomas Southwood Smith.

According to King, the 1840s represented a turning point in the change from eighteenth-century rationalism to nineteenth-century empiricism. As King states, "By the later eighteenth century, upheavals were affecting virtually every branch of human endeavor, not only medicine but art, music, philosophy, law, literature, politics, and economics. As we pass from the era of Benjamin Rush to that of William Osler," King declares, "we also pass from Haydn to Mendelssohn to Debussy, from Benjamin West to Delacroix to Monet, from cottage industry to factory production, from the oil or kerosene lamp to electricity, from the horse and wagon to the horseless carriage. Massive changes affected every cultural modality" (p. 3). The world was undergoing dramatic change, and in this excellent book King explains the process of change in medical thought from the eighteenth-century physicians who merely observed what was happening, to those of the modern era who were willing and able to experiment in order to seek new evidence. John Harley Warner's book, *The Therapeutic Perspective: Medical Practice, Knowledge, and Identity in America, 1820–1885* (1986) provided specific information on how medical practice changed during the nineteenth century. Now, thanks to King, we know precisely why it changed, and why it changed in the way it did.

In many ways, this book also provides an indication why King's previous works, especially *The Medical World of the Eighteenth Century* (1958), have become classics in the field of the philosophy of medicine as well as the history of medicine. For persons interested in the history of medicine, the philosophy of medicine, and the world of the eighteenth and nineteenth century, this is an important work, one that will be cited for generations to come. King is to be applauded for a job well done.

MARTIN KAUFMAN
Westfield State College

KENNETH ALLEN DE VILLE. *Medical Malpractice in Nineteenth-Century America: Origins and Legacy*. (The American Social Experience Series, number 19.) New York: New York University Press. 1990. Pp. xvi, 319. $34.95.

This first full-length history of medical malpractice seeks to trace the roots of the current malpractice crisis, rather than attempting to survey the entire subject. Kenneth Allen De Ville sees 1835 to 1865 as the critical takeoff period, when the number of suits exploded and the medical profession was launched toward its present predicament. The initial sparks included antiprofessionalism, intraprofessional wrangling, and physician oversupply. But the trajectory persisted long after these forces were spent, fueled by such fundamental cultural transformations as the decline of community methods for resolving disputes, a new preoccupation with health, and a greater willingness to blame illness on human error rather than Providence.

From appellate case reports, medical journals, and previous historical articles, De Ville constructs the best estimate to date of quantitative trends in malpractice litigation. But the interpretation of these figures is somewhat clouded by methodological imprecision. The book tabulates sixty-six "representative" judgments, and lists half of the other appellate cases the author located, but does not explain by what sampling techniques these particular examples were selected. And, while the number of malpractice cases soared from 1835 to 1865, so did the number of physicians. The author provides separate noncomparable estimates of each increase, but does not calculate how much the number of suits per physician actually changed, or whether there was a similar increase in nonmedical tort litigation.

De Ville generally succeeds at the difficult task of

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linking technical developments in medicine and law to larger cultural changes, although some significant issues are slighted. His portrait of colonial community self-regulation looks more like Michael Zuckerman's "peaceable kingdoms" than Helena Wall's "fierce communion." By oversimplifying Jacksonian views on professionalism, he erroneously labels homeopaths working-class egalitarians. And he invokes persistent faith in Providence to explain the paucity of antebellum southern cases, without examining whether southern concepts of malpractice might also have been influenced by slavery.

One particularly original and provocative chapter documents a mid-nineteenth-century effort to base medical duty on contract rather than tort principles. The author speculates that a contract system might have permitted doctors to negotiate reduced levels of liability, although the weak bargaining position of most physicians at the time makes that outcome seem unlikely.

Applying the legal standard of "ordinary care" was extremely difficult for nineteenth-century courts because physicians disagreed over the value of most medical procedures. De Ville provides an insightful, nuanced exploration of the complex relationship between therapeutic controversy and malpractice. But he overlooks other types of malpractice cases, such as those based on lack of patient consent.

De Ville writes lucidly about complex technical issues, but this book is rather poorly edited. And, while he is a historian, he often shares the perspective of his medical sources, repeatedly calling malpractice allegations a "plague" that posed a "menace" to doctors.

On balance, this volume's virtues clearly outweigh its faults. De Ville provides a valuable, long-needed starting point for understanding a significant aspect of medical, legal, and cultural history.

MARTIN S. PERNICK
University of Michigan, Ann Arbor

PHILIP R. REILLY. *The Surgical Solution: A History of Involuntary Sterilization in the United States*. Baltimore: Johns Hopkins University Press. 1991. Pp. xvi, 190. $19.95.

This book by Philip R. Reilly, the first critical history of involuntary sterilization in the United States, traces the practice from its intellectual origins in the writings of Charles Darwin and Francis Galton to the present. The study's minor flaws are outweighed by its many strengths, including superb coverage of the heretofore almost unrecognized persistence of forced sterilization programs after World War II.

Reilly concentrates on the decades after 1907, when Indiana enacted the first eugenic sterilization law, and on the impact of such legislation on institutionalized persons deemed mentally retarded. Chapters covering the evolution of eugenic ideas and the eugenics movement are followed by a review of early efforts to apply "the surgical solution." These three initial chapters, while useful, mainly summarize previous research. Subsequent chapters draw increasingly on primary sources (such as the papers of the Eugenics Record Office and Harry L. Laughlin), bringing important information to light and contradicting received wisdom on American reactions to Nazi sterilizations.

One of these chapters deals in detail with the enactment of the first wave of U.S. eugenic sterilization laws; another centers on Laughlin, whose work helped persuade legislators, philanthropists, and Supreme Court justices of the need for reproductive controls. Thereafter Reilly chronicles the post–World War I resurgence of interest in sterilization (a revival fueled, he shows, by the Supreme Court's *Buck v. Bell* [1927] decision) and the simultaneous growth of anti-eugenics. Sterilizations declined during World War II—in large part, Reilly argues, due to the "shortage of civilian physicians" (p. 95)—only to pick up again during the allegedly "quiet years" of the 1950s and 1960s. Reilly relates how, during these "quiet" decades, eugenicists regrouped to support the nascent "voluntary" sterilization movement. Interestingly, it was the 1973 *Roe v. Wade* abortion decision that finally terminated legal struggles over elective sterilization, including women's efforts to be sterilized without spousal consent. The concluding chapter documents involuntary sterilizations of the 1970s, raises the issue of whether mentally retarded persons have a "right to be sterilized," and discusses India's and China's use of coercive sterilization to control population growth.

The author's background—he holds M.D. and J.D. degrees and directs the Shriver Center for Mental Retardation—enables him skillfully to discuss the medical and legal aspects of involuntary sterilization; but it also accounts for some of his weaknesses as a historian. The early chapters of the book are marred by numerous inaccuracies (for example, the Newark Custodial Asylum for Feeble-minded Women was founded by Josephine, not Amy, Shaw Lowell; the professional association of superintendents of mental retardation institutions formed in 1876, not 1877; and Richard Dugdale could not possibly have entertained "a Lombrosian notion" [p. 9] in 1874, several years before Lombroso's first book on criminal anthropology appeared in Italian). More importantly, Reilly seems unaware of Isaac N. Kerlin's sterilizations of retarded women at the Elwyn, Pennsylvania, institution in the early 1890s—probably the first such operations in the United States and procedures that profoundly impressed Kerlin's fellow superintendents. As a result, Reilly inaccurately holds that "Early discussion of eugenic sterilization focused largely on men" (p. 34). In addition, he pays little attention to prophylactic institutionalization, the method of de facto sterilization that most eugenicists

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favored over surgery. In consequence, Reilly minimizes the extent to which eugenicists themselves led the opposition to surgical sterilization.

The book's drawbacks are eclipsed, however, by its many achievements. Using guides to medical and popular journals, Reilly traces the spread of pro-sterilization sentiment. He documents shifts in the rationales used by involuntary sterilization's proponents and changes in the regions where the practice predominated over time. Although he slights early eugenicists' intense interest in the reproductive incapacitation of women, he is sensitive to gender differences in sterilization rates in the post–1907 period, providing our first clear evidence that females, although often outnumbered in institutions for retarded persons, were disproportionately sterilized. He shows that Nazi sterilization efforts fed on the work of Laughlin and other U.S. eugenicists and that "Revulsion over Germany's racist politics did little to curtail American programs before or after World War II. Indeed, American advocates pointed to Germany to illustrate how an enlightened sterilization program might quickly reach its goals" (p. 95). Finally, Reilly makes us realize that involuntary sterilization continues to attract supporters today, in the United States and throughout the world.

Nicole Hahn Rafter
Brookline, Massachusetts

Jane Maienschein. *Transforming Traditions in American Biology, 1880–1915*. Baltimore: Johns Hopkins University Press. 1991. Pp. xi, 366. $48.00.

The subject of this book by Jane Maienschein is the so-called "revolt from morphology," which transformed experimental biology between 1880 and 1915. In this period biologists abandoned the belief that phylogeny, rather than hereditary "factors" or physiology, could explain morphogenesis. Biologists also experienced a rapid differentiation of semi-independent experimental disciplines (for example, cytology, embryology, and genetics). It was a formative movement, central to the history of modern science.

Methodologically, this book represents an effort to move from individual biography to a more synthetic treatment. The project began as a biography of embryologist Ross Harrison and his role in the revolt from morphology, but along the way it occurred to Maienschein that the fundamental issues of Harrison's work might better be explored in a group biography of Harrison and others who labored in different fields. Maienschein chose four subjects: Harrison, the cytologists Edwin Conklin and Edmund B. Wilson, and Thomas Hunt Morgan of genetics fame, and uses their lives and work as a vehicle to explore the larger movement in which the four were leading figures.

The structure of Maienschein's book reflects her dual purpose of group biography and overall survey. The first two chapters cover the morphological and physiological schools of Johns Hopkins University, where Wilson, Conklin, Harrison, and Morgan were trained. Three chapters survey experimental biology in Europe, using the four friends' postdoctoral tours as a narrative framework and showing how they assimilated and then transcended the new European trends. The final four chapters take up the careers of each of the four subjects, describing in some detail the work they did in their chosen specialties to about 1915.

In many ways this is an admirable book, but in some important ways it falls short of what the author hoped to do. Using group biography is a bold and laudable attempt to cover a stretch of ground without falling back on straight biography. Maienschein's choice of subjects, however, makes it more awkward to work in other equally essential players such as Charles O. Whitman and Jacques Loeb. Also, Maienschein does less than she might have to exploit the dense fabric of intellectual and social connections between her chief protagonists. The four chapters on Harrison, Conklin, Wilson, and Morgan read too much as though they led parallel but separate careers.

While Maienschein provides a useful summary of what we know of biologists' institutions, conceptions, and achievements between 1880 and 1915, she offers few insights into the fundamental causes of scientific change. Why was the old evolutionary morphology abandoned? What drove the rapid speciation of new experimental disciplines? We know what happened but not why. As the author says, she chose a morphological and descriptive approach over a physiological and functional one (p. 9).

The author puts much weight on the influence of Johns Hopkins' physiologists, but the evidence of specific connections is scanty. Analysis of the changing role of physiology within the old morphological practice might have been more fruitful (used at first to explain away unwelcome anomalies of development, it gradually took over). Helpful, too, might have been a closer analysis of morphologists' actual experimental practices and tribal customs. Maienschein is aware of the newer historiography of laboratory practice but invokes it more than she does it. I am persuaded of the author's view that the old morphology was not abandoned but transformed. But is this view so very different from the standard view offered by Garland Allen, against which Maienschein sets herself?

Those interested in the history of modern biology will want to read this informative and well-written book. But historiographically it is cautious and stands uneasily between old and new.

Robert E. Kohler
University of Pennsylvania

Henry L. Minton. *Lewis M. Terman: Pioneer in Psychological Testing*. (American Social Experience Series,

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number 11.) New York: New York University Press. 1988. Pp. xiv, 342. $40.00.

PAUL DAVIS CHAPMAN. *Schools as Sorters: Lewis M. Terman, Applied Psychology, and the Intelligence Testing Movement, 1890–1930.* (American Social Experience Series, number 12.) New York: New York University Press. 1988. Pp. xv, 228. $40.00.

Lewis Madison Terman (1877–1956) transformed American educational psychology, fixing its structural relationship to school practice. Lewis who? Editors at New York University Press have recognized Terman's significance by delivering two books within the same year on this entrepreneur of the intelligence testing movement who revised the Binet-Simon Intelligence Scale as the Stanford-Binet Intelligence Test. Both works place Terman, a psychologist who spent most of his career at Stanford University, in the context of Progressive-era educational philosophy and school policy. Neither, however, moves very far beyond the standard historiographic literature on Progressivism and the history of educational reform; both works contribute significantly to the existing literature rather than reframe any of its questions or assumptions.

Henry L. Minton's volume is a closely observed account of Terman as a man, in his capacity as scholar, academic politician, and lover. Minton examines Terman's entire psychological corpus, giving equal attention to the work outside intelligence testing and the longitudinal study of the gifted, for which Terman is best remembered. Woven through the account are details of Terman's sexual persona, which are held in tension against Terman's preconceptions about gender differences. Minton is not always explicit in connecting his psychobiographical evidence to Terman's scholarly activities, and the cumulative effect is suggestive, in both a historical and a prurient sense. The reader is left with a strong impression that Terman's own mildly pathological (or at least destructively self-indulgent) heterosexuality, which Minton deftly portrays, largely determined his work on masculinity and femininity, which relied experimentally on a distinction between "active" (read "masculine") and "passive" (read "feminine") male homosexuals. Yet Minton brings little social context to these profoundly social questions, affirming only that Terman's work mirrored prevailing social norms. Inasmuch as Terman conducted and published his studies during the late depression years, when conventionally defined masculinity was affronted both by mass unemployment and by women's increasing political participation, these norms were by no means fixed. Moreover, Terman and his colleagues helped produce the very social-scientific debates that in turn shaped these sexual norms. This broader context, difficult to provide in the biographical format, might help place Terman and his work in a more dynamic relation to the cultural and political institutions that defined them. Nonetheless, Minton does best what his format is designed to do: to paint a portrait in character. Minton's own training in psychology gives him a special acuity in observation, yet this asset also prevails over the historian's predilection toward social explanation.

Paul David Chapman has written the work of a historian and school administrator; it is about context, politics, and social explanation. Rich in detail yet carefully structured, the book uses Terman, his local school reform activities in the San Francisco Bay Area, and the national intelligence testing movement to examine concentrically the interplay between culturally constructed social problems and personal, professional, political, and intellectual interests. Chapman uses biography as metonym for telling a much larger story and is successful in demonstrating the pivotal intellectual and political role of Terman and his colleagues in creating a durable administrative structure within the public school system that contemporaries viewed as a "selective agency" for the national purpose. This is an admirable book: clear, well researched, and insightful.

Chapman is even-handed over the most volatile policy issues, without begging the ethical and political questions provoked by the testing enterprise, then and now. This assessment is partially a result of extensive research on school administration and the implementation of mental testing, where Chapman makes his greatest contribution. Yet he is not emboldened by his own impressive evidence to move much beyond conventions of Progressive historiography as set forth in the 1960s and 1970s by Lawrence Cremin, David Tyack, Raymond Callahan, and Robert Wiebe.

Mental testing as a basis for school reorganization is seen as an example, not a component part, of Progressive reform. What Chapman has uncovered about it he does not bring to bear in redefining the whole of Progressive ideology, vocabulary, or practice. The schools remain reflections of the needs of a larger society, as though they were neither an integral part of that society nor a central preoccupation among Progressive activities. Their dynamic role in producing and replicating the social order is not fully explored.

This is asking a lot of a very cogent study, but the complaint can be generalized to any of the subdisciplines in history that are subject to another profession or social science, for example the history of education, history of economics, or history of medicine. The institutional walls around the discipline, and the professional territory that its historians are expected to respect, if not defend, become self-maintaining, appearing as though they had always existed in their present form. This illusion, while powerful in supporting the political autonomy of the profession or discipline under study and helpful in titling and marketing books, is much less conducive to any integrated historical understanding of the way in which apparently autonomous institutions—including disciplines and professions—emerge out of prior

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social relationships. The politics, in the sense of shifting power relations that entail intellectual, social, economic, cultural, and personal commitments, can disappear next to the imposing solidity of formal institutions and identifications. The authors and editors of a growing literature on the interstices among disciplines and professions—Andrew Abbott, *The System of Professions* (1988); Dorothy Ross, *The Origins of American Social Science* (1990); Gerald Geison, ed., *Professions and Professional Ideologies in America* (1984)—have begun to break down the retrospective partitions between disciplines in the early stages of their definition and to use historical inquiry into social theory as a way of understanding and theorizing about social change.

Despite these caveats, both works are important to the historiography of the Progressive Era and its psychology. Chapman writes of social policy and school politics with compelling clarity, while Minton creates an intimate portrait of Terman's psychology. Together these books give a good sense of Terman's location in the politics of Progressive school reform and the history of American psychology.

JoAnne Brown
Johns Hopkins University

James Rodger Fleming. *Meteorology in America, 1800–1870*. Baltimore: Johns Hopkins University Press. 1990. Pp. xxii, 264. $45.00.

James Rodger Fleming divides the history of meteorology in the United States into four major periods: that of individual diarists before 1800; of emerging systems and expanding horizons between 1800 and 1870—the specific focus of this study; of government service, 1870 to 1920 (and beyond); and the current professional period, which began in the 1920s. Fleming concedes the fact that the history of meteorology can boast of no crucial experiments or individuals of genius to compare with the likes of Isaac Newton, Antoine-Laurent Lavoisier, Charles Darwin, Louis Pasteur, or Pierre Curie and Marie Curie, but he will not accept the harsh indictment of one eminent scholar who branded generations of meteorologists Baconians while dismissing the entire theoretical structure of the atmospheric sciences as useless. The popular perception of meteorology, centered on the television forecaster, has further confused the issue in the minds of scholars who should know better. "Weather reporters," Fleming reminds us in his spirited introduction, "are communications specialists, not atmospheric scientists. They deal with a public interested in whether a coat or an umbrella will be needed, not in the principles governing the general circulation of the earth's atmosphere" (p. xx–xxi).

As a means of providing a corrective, Fleming has plumbed the extensive but highly scattered and disorganized literature of the nineteenth century, consisting mainly of brief chronologies, individual diaries, newspaper reports, telegrams, pamphlets, and correspondence. At the beginning of the period the typical observers were equipped only with a thermometer, wind vane, and rain gauge. They usually observed on an irregular basis and communicated the results, when they did so at all, by mail. After 1850, however, the Smithsonian supplied certain observers with calibrated instruments, supported observations of the upper air at mountian stations and by balloonists, and introduced standardized reporting forms and schedules. Reporting by telegraph replaced the mail. Even the weather cooperated, typically traveling from the West or the South toward the more densely populated Atlantic coast, where storm warnings were regularly posted.

Equally important was the development of theoretical models by scientists searching for the causes of those storms. Fleming details the "American storm controversy," which set the tone of the meteorological agenda for decades to come. Its rival protagonists—William C. Redfield, James Pollard Espy, and Robert Hare—argued vociferously over the primum mobile of storms: was it gravity, caloric, or electricity? The question was not resolved in their lifetimes and neither were the arguments over methodology, but the great debate proves beyond reasonable doubt that science in all of its manifestations—hypothesis and theory, observation and experimentation, induction and deduction—was part and parcel of the meteorologists' repertoire. Detailed and handsomely illustrated, this work is an original contribution to the history of nineteenth-century science.

Gale E. Christianson
Indiana State University

Maila L. Walter. *Science and Cultural Crisis: An Intellectual Biography of Percy Williams Bridgman (1882–1961)*. Stanford: Stanford University Press. 1990. Pp. x, 362. $42.50.

In the mid–1920s, Harvard physicist Percy Bridgman established himself as a leading American interpreter in the cultural assimilation of Albert Einstein's special theory of relativity with the publication of *The Logic of Physics* (1927), his most enduring and popular publication. His philosophy of science, "operationalism," joined pragmatism and behaviorism as part of American empiricism and defensively accommodated the revolution in theoretical physics. Bridgman extended operationalism to treat social and political issues. While many pragmatists and behaviorists supported social democracy, Bridgman pushed operationalism toward individualism and opposition to collectivistic philosophies of the 1930s. An essentially shy and private man, Bridgman did not see himself as a popularizer of science—"I think that perhaps more than most I write to clarify my own ideas" (p. 131)—but he did have a mission. He spoke for science as "the emancipator of mankind" from the "oppressive

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and false authority" of religion, and as the chief protector of individualistic freedom (pp. 256–57). His biographer, Maila L. Walter, characterizes his philosophy as similar to existentialism in its emphasis on conscious act as the central instrument by which a person builds a meaningful life. When faced with inoperable bone cancer, Bridgman requested his physician's assistance in euthanasia. Denied this mercy, he committed suicide.

Einstein's special theory provided the focus for *The Logic of Modern Physics*, but Walter locates the conceptual origin of operationalism in Bridgman's ruminations about dimensional analysis, over which a controversy dragged on from 1909 through the early 1920s and led to Bridgman's first publication, *Dimensional Analysis* (1922). In a brilliant exposition forming the core of her intellectual biography, Walter demonstrates how operationalism emerged from a formal conceptualization (provided by Bridgman's position in the dimensional analysis controversy) of his professional experience as an American experimental physicist, especially from his highly developed physical intuition and rich fabric of tacit procedures that resulted from the lack of theory to guide his work in high-pressure phenomena. This background led Bridgman to read Einstein's special theory as a tract in procedural empiricism. Bridgman was annoyed by A. S. Eddington's absolutist interpretation of Einstein's theories, which Bridgman saw as implicitly mystical. He wrote *The Logic of Modern Physics* to quash the incipient absolutism and transcendentalism that he feared Eddington's view might bring back to physics, or that might encourage the public to believe it had the blessing of physical theory.

Walter argues that Bridgman's reading of the special theory was flawed. According to Walter, special relativity's main philosophical message concerned a theory of physical communication rather than an operational philosophy of science. Bridgman's operationalism was tied to a congruence theory of measurement rather than a communication theory of measurement, and hence it was fundamentally anchored in classical rather than Einsteinian epistemology. I use "flawed" rather than "wrong" to characterize Walter's assessment because she sees the origin of Bridgman's misreading mainly in a character defect rather than in an intellectual incapacity, although analysis of his intellectual limitations is central to her exposition. Bridgman's father represented the spirit of New England Puritanism and pressed his views on his son. Bridgman overreacted to his father's religion (p. 197). He developed an antireligious attitude and antimetaphysical ideology that blinded him to a postmodernist understanding of the necessary role of background ontology in establishing the intelligibility of physical theory. His posture was ironic because the pattern of his accommodation to twentieth-century physical theory unknowingly mimicked the "rationale and value" of ascetic New England Protestantism (p. 2). He became a scientific Puritan whose operationalist philosophy was essentially a moral vision of human limitation and humility.

The balance of Walter's well-written and insightful account of Bridgman's intellectual career is devoted to the fortunes of operationalism among American scientists and philosophers. Bridgman thought the philosophy was misinterpreted, even by those who supported it. Responding and clarifying, he shifted his philosophy toward subjectivism and ethical nihilism. As he treated each new subject—quantum mechanics and thermodynamics, as well as political and social ideas—with operationalism, Bridgman isolated himself. Other thinkers were unable to extend his insight into technical philosophy. When religious conservatism revived after World War II, his antireligious views seemed increasingly at odds with the temper of the times. In an exchange with the Catholic philosopher Jacques Maritain, Maritain's neo-Aristotelianism seemed, Walter comes close to saying, more reasonable and intellectually more powerful than Bridgman's spare scientific subjectivism.

Walter's provocative biography of Bridgman is extraordinarily valuable for American historians. She moves the story of American physics beyond the contributions of the theoretical innovators and social history. Each chapter opens new avenues for historians of ideas to follow, especially Bridgman's controversies with other American physicists and the retreat from scientific naturalism after 1945. Scholars will not, however, so willingly follow her personal interpretation of Bridgman's life, which she portrays as a tragedy, although she recognizes that Bridgman himself would not have said his life was tragic or meaningless. To accept her interpretation in the final chapter, titled "The Broken Covenant," requires judging Bridgman's life from the point of view of Christianity. The necessity for and legitimacy of this perspective are not developed in the preceding scholarship (aside from a hint in a brief quotation of Bridgman's father); rather, the perspective appears *ex cathedra* in her judgment of Bridgman's suicide and his life as summed up in his final act.

RONALD C. TOBEY
University of California, Riverside

DAVID MCBRIDE. *From TB to AIDS: Epidemics among Urban Blacks since 1900*. (SUNY Series in Afro-American Studies.) Albany: State University of New York Press. 1991. Pp. x, 234.

David McBride has written an ambitious study of the public policy and sociomedical debates surrounding black health in the United States from 1900 through the late 1980s. His book focuses on three basic questions: What epidemic diseases most gravely affected the health of black Americans during this century; why medical and political leaders in both the white and the black communities perceived and responded

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to black health problems as they did; and how these sociomedical debates have influenced the development of modern society.

Tuberculosis posed the gravest threat to the health of black Americans in 1900, and from all available data, blacks suffered a much higher incidence of TB than whites. In the years between World War I and World War II, the epidemic disease that caused the most concern about the health of black Americans was syphilis. Again, medical authorities assembled impressive data to show a much higher incidence of syphilis in blacks than in whites. In the decades following World War I, TB and syphilis appeared to be under control, but during the 1980s a new disease burst on the scene. AIDS confronted Americans with a terrifying epidemic, and once again medical authorities were quick to point out that black Americans suffered a much higher incidence of the disease than whites.

One of McBride's most valuable contributions is his analysis of how different groups concerned with black health attempted to explain why various epidemics hit blacks harder than whites. In broad terms, he identifies two competing epidemic paradigms. The first, which McBride labels "sociomedical racialism," sought to explain epidemiological patterns in terms of racial traits, a feat of reductionism that allowed many white physicians to elevate social attitudes to scientific verities. The second paradigm, which McBride calls scientific epidemiology, was rooted in a multifactorial approach that stressed genetic and environmental differences between blacks and whites. McBride builds a strong case for his argument that much of the confusion that has marked the current debate over how to respond to AIDS in the black community stems from the failure to heed the lessons of the past.

Despite its attributes, this work has several weaknesses. Because the topic is vast and the pertinent monographic literature is meager, McBride is not able to deliver everything he promises. Some topics are glossed over while others are barely mentioned. For example, he does not pay sufficient attention to the origins and development of health legislation, and in most instances he is not able to reconstruct how the competing paradigms he outlines shaped health policy or programs. Moreover, one can only hope that McBride will be better served by his editors in the future: grammatical errors, social-science jargon, and awkward sentences abound. Still, this book is a brave effort to fill a major void in the literature on the health problems of black Americans, and it deserves to be read widely.

JAMES H. JONES
University of Houston

EARL LEWIS. *In Their Own Interests: Race, Class, and Power in Twentieth-Century Norfolk, Virginia*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xv, 270. $34.95.

Black urban history has matured significantly since Gilbert Osofsky and Allan H. Spear revived interest in the field in the 1960s. They focused on ghettoization and race relations conflicts in northern cities, which are indeed worthy subjects, but more recently scholars such as Joe W. Trotter, Jr., have identified the movement of African Americans into the industrial-capitalist work world as more significant in shaping their lives and actions. Still, southern urbanism was neglected. Now Earl Lewis suggests a focus on empowerment as blacks acted "in their own interests."

No different from other folk, African Americans in Norfolk sought neighborhood betterment, equal rights before the law, and economic improvement through meaningful jobs in a modernizing economy. When some of these goals seemed unattainable, others assumed priority. With loss of the franchise at the turn of the century and isolation in a racially stratified job market, black Norfolkians campaigned for improved city services so as to strengthen their community and also reclaim a political voice. The World War I era briefly posed new alternatives, with Marcus Garvey's black nationalist movement attracting considerable support in Norfolk. But this highlighted class divisions within a community that was no more monolithic than other ethnic groups.

The Great Depression brought both greater impoverishment and new avenues for protest and advancement. Coalitions with white workers produced some economic benefits but did not lead to political gains. World War II was more pivotal. Interracial labor alliances stimulated whites "to move the language of competition beyond black versus white to haves versus have-nots." At the same time participation in the war sharpened African Americans' focus on the unfulfilled promises of democracy. Although this helped lay a foundation for the civil rights movement, the campaigns of the 1950s and 1960s focused too narrowly on political and social improvements while neglecting economic issues.

Lewis sees Norfolk as representative of other middle-size southern cities where changing class and race factors dictated varying responses in different eras. His thesis is that African Americans were consistently proactive in pursuing "their own interests" with a realistic conservatism, not committing wholeheartedly to chimeras like Garveyism or communism, and justifiably skeptical of the sincerity of white workers.

This study is a useful addition to the black urban history bookshelf as well as a companion to Henry Lewis Suggs's recent biography of black Norfolk publisher and community leader P. B. Young (*P. B. Young, Newspaperman: Race, Politics, and Journalism in the New South, 1910–1962* [1988]). While other southern cities deserve similar attention, it is also time for historians to weave insights from the last thirty years of research into a comprehensive synthesis of the

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African-American urban experience from its colonial origins to the present.

THEODORE KORNWEIBEL, JR.
San Diego State University

ANTHONY M. PLATT. *E. Franklin Frazier Reconsidered.* New Brunswick, N.J.: Rutgers University Press. 1991. Pp. xi, 278. $27.95.

In the 1940s and 1950s, E. Franklin Frazier was considered the leading "Negro sociologist" in the United States. He studied sociology first with Frank Hankins at Clark University, receiving his masters degree in 1920; later he studied with Robert K. Park and Ernest Burgess at the University of Chicago. Frazier's Ph.D. dissertation was published in 1932 as *The Negro Family in Chicago.* Along with a spate of scholarly and popular essays and reviews, Frazier published *The Negro Family in the United States* (1939), *Negro Youth at the Crossways* (1940), *The Negro in the United States* (1949), *Black Bourgeoisie: The Rise of a New Middle Class in the United States* (1957), and *Race and Culture Contacts in the Modern World* (1957). In 1948 he became the first African American to be elected president of the American Sociological Society, and between 1951 and 1953 he served as the Chief of the Division of Applied Social Sciences at UNESCO headquarters in Paris.

In 1965, however, Daniel Patrick Moynihan cited Frazier's works to support his controversial conclusions in the government report, *The Negro Family: The Case for National Action.* Using quotes from Frazier, Moynihan concluded that the African-American family was weak, matriarchal, highly unstable, and embedded in a "tangle of pathology." The "Frazier-Moynihan thesis" became the subject of widespread scholarly debate in the 1960s and 1970s and still has some adherents within the sociological establishment.

Anthony M. Platt's book is a self-consciously revisionist work that not only exposes the significant differences between Frazier and Moynihan's perspectives on the black family but also presents a sympathetic portrait of a complex man and committed scholar whose values and principled positions often got him into trouble. Using Frazier's published and unpublished writings, his personal papers, as well as interviews with former students, colleagues, and friends, Platt provides a detailed account of the trials and tribulations of a black academic who recognized the political responsibilities of scholars and intellectuals.

Although he was a loner and rarely joined formal organizations and institutions, Frazier was an activist who made important ideological commitments. He advocated socialistic economic approaches to the problems of poverty in the United States but never was formally affiliated with the Socialist or Communist parties. Frazier challenged biological or genetic interpretations of social behavior and emphasized environmental factors. He considered himself a gadfly, a critic, a "disturber of the peace," and when confronted by the congressional investigators about his "Communistic sympathies" and "un-American activities," he defended himself and his principles.

Although Platt's study provides keen insights into the life and work of this complicated intellectual, there are many topics barely touched on and in need of further study. Frazier's most important scholarly debate with anthropologist Melville Herskovits over the African origins of African-American cultural practices and institutions is only casually mentioned. There is no real discussion of the influence of Frazier's travel and research in Brazil, Central America, and the Caribbean on his later works. Platt has concentrated on Frazier's earlier, most productive years; but by the 1950s he had changed many of his previous views, especially about the eventual assimilation of blacks into white society. Platt's volume is a welcome addition to the growing literature on African-American intellectuals in the twentieth century, but it would have been more useful had the author included a bibliography of Frazier's works (or at least the works cited) and full references in the endnotes for each chapter.

V. P. FRANKLIN
Drexel University

KEITH L. BRYANT, JR. *William Merritt Chase: A Genteel Bohemian.* Columbia: University of Missouri Press. 1991. Pp. xiv, 325. $35.00.

Carefully examining the primary sources in the Chase Archives at the Parrish Museum in Southampton, New York, and the Archives of American Art, Keith L. Bryant, Jr., has researched the life of the American Dark Impressionist painter, William Merritt Chase (1849–1916). He provides many details of the artist's early life in his native Indiana, of his academic training in Munich, and of his subsequent success with his painting and teaching in New York and on Long Island. We learn who the artist's friends, associates, and students were; what pictures were submitted to each exhibition, what the critics said of his work; how his methods and outlooks changed through the years; what he urged of his students; what his house at Shinnecock, Long Island, looked like; and what his daughters said to him on this or that occasion. Indeed, there are some details of family life that have the tone of a novel. It is uncertain how Bryant could have known all that he writes; consider, for example, this statement: "Chase helped the baby take her first few steps while Alice, kneeling on the floor, reassured Cosy that she would not fall" (p. 107).

Ultimately, the book is not entirely satisfying, in spite of a great amount of valuable research. There are no color illustrations (evidently to keep costs down), so a proper appreciation of the work cannot be gained. The discussions of the pieces (usually

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descriptions of the scene and enumeration of details) often become drowned in a welter of quotations and biographical facets. The reader is not convinced that Chase is an important painter, despite the author's repeated assertions to that effect. Toward the end of the book, countering the writer Huntington Wright's denigration of Chase, Bryant points out that the negative view "flew in the face of significant evidence that Chase was one of a small number of nationally and internationally recognized American masters" (p. 217). True enough, but why he should have become this is not sufficiently explored. One part of Chase's importance is brought out clearly. Among his students, working in a different style, were some who became leading American Modernists, among them Charles Sheeler, Arthur B. Carles, Charles Demuth, and Morton Schamberg.

ABRAHAM A. DAVIDSON
Temple University

BRUCE CLAYTON. *W. J. Cash: A Life*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1991. Pp. xiv, 236. $24.95.

In 1940, after writing and struggling a decade, W. J. Cash finished his only book; the next year, he committed suicide, just after its publication. In 1965, at the height of the civil rights movement, and hence in the midst an intense interest in the American South, Alfred A. Knopf proposed that historian and journalism professor Joseph L. Morrison write a biography of the man whose book, *The Mind of the South*, still intrigued scholars and serious readers a quarter century later. Although not footnoted, Morrison's *W. J. Cash: Southern Prophet; A Biography and Reader* (1967) has stood well the test of time. Now, after another twenty-five years, Bruce Clayton pays another tribute to Cash's "dazzling scope" and "extraordinary talent" in a distinctive and excellent biography.

Clayton's work excels in three areas. First, Clayton vividly evokes the world in which Cash lived. Second, he conducts a thorough analysis of other literature of the period. Finally, he concisely explains Cash's life. Well researched and documented, this book is a joy to read. Clayton examines at length Cash's boyhood and adolescence, detailing his middle-class, white southern racist background and his liberating years at Wake Forest. Wisely, Clayton lays out the structures, but in the best humanistic tradition does not deal directly with the psychological dynamic driving the author, for whom his most celebrated critic, C. Vann Woodward, wished "Peace to his troubled spirit" (*American Counterpoint: Slavery and Racism in the North/South Dialogue* [1971]).

According to Clayton, Cash had two major contentions: one was continuity of the southern mind in the Old and the New South, while the other was that fabled planters, the so-called aristocracy, were in reality aggressive farmers carving homesteads out of the frontier. Clayton summarizes Cash's thesis as the representation of a distinct southern culture or mind with a Freudian defense mechanism to protect itself from "the Yankee mind, the Modern Mind, and the Negro" (p. 93).

In discussing Cash's "Moving Row" column in the small-town *Cleveland Press*, Clayton assesses Cash's writings on race as humane and sensitive and his analysis of politics as insightful. He credits some of Cash's columns as prophetic.

Clayton concentrates on Cash's life as an intellectual. At Wake Forest, Cash admired President William Louis Poteat as a religious and rational man at a time when that combination was unpopular. In revering thoughtful analysis as the right and proper thing to do, Cash moved away from being a clone of H. L. Mencken to developing his own more empathetic analysis, thereby raising the caliber of *The Mind of the South*. Yet devotion to critical reflection, Clayton implies, contributed to Cash's mental fatigue and neurasthenia.

Clayton ends his book with an insightful historical perspective on *Mind of the South*. In this critique, Clayton supports Cash's line of reasoning and his descriptions of paradoxes and ironies. He nevertheless admits that Cash was prone to sentimentality regarding the southern textile industry's altruism. Since Cash's father rendered some assistance in the founding of mills, Clayton believes that Cash may have been honoring his family.

Clayton's life of Cash is a thoroughly enjoyable study. The reader may even chuckle aloud at Cash's explanation as to why William Faulkner and Erskine Caldwell were "old-fashioned 'romanticists' of the South in reverse" (p. 119). This book is essential for anyone interested in *The Mind of the South* or in southern journalism and intellectuals during the 1920s and 1930s; it is highly recommended for everyone else.

VERNON BURTON
University of Illinois, Urbana-Champaign

BRUCE J. SCHULMAN. *From Cotton Belt to Sunbelt: Federal Policy, Economic Development, and the Transformation of the South, 1938–1980*. New York: Oxford University Press. 1991. Pp. xii, 333. $35.00.

To paraphrase a sardonic comment originally directed at Brazil, the South is the region of the future—and always will be. Indeed, even after 100 years of convergence on national norms, the South still finds itself significantly below the "American standard" in most measures of socioeconomic development. It is the latter half of this period of convergence that Bruce J. Schulman deals with in his provocative new book.

What impresses Schulman most about the Sunbelt today are its many shadows. He is particularly struck

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by the degree to which southern living standards lag behind those in other parts of the United States. Whatever success the region has had in modernizing its infrastructure over the years, the southern population, especially the large black minority in the region, still compares quite unfavorably to the U.S. population as a whole in terms of per capita income, the incidence of poverty, infant mortality, life expectancy, literacy, high school graduation rates, and so on.

The situation in which the contemporary South finds itself was not foreordained, however. For a time in the 1930s and 1940s it seemed as though the South was following a developmental path that Schulman believes had much promise. To understand how and why the South turned away from this path one must, according to the author, understand the ways in which the federal government shaped the region's development efforts over time. As told by Schulman, it is a story of seduction and betrayal as much as anything else, and it begins in the 1930s when the South was considered the nation's top economic problem.

The plot of this story is complex but rendered in a straightforward manner. After an initial reluctance to act decisively against poverty and underdevelopment in the South, Franklin D. Roosevelt, beginning in 1938, "launched a series of aggressive programs to reorder the southern economy" (p. viii). Such federal programs—the Fair Labor Standards Act of 1938 is a particularly apt case in point—were both broad in scope and (in an American context) redistributionist in effect. Considered *in toto*, they seemed to be sparking not only economic recovery but also the beginning of a profound social transformation in the South before they were interrupted, scaled down, redefined, or, in some cases, abandoned altogether, because of the exigencies of World War II.

To be sure, the coming of war did not mean that the federal government's overall role in the South would shrink. Schulman is quick to point out that the federal government was to become if anything even more important in shaping the southern economy in the years after 1941. The problem, according to the author, was related to the way in which the federal government's presence—and political allies—in the South changed over time.

The broad welfare and regional development programs for the South and the liberal policy makers who designed and oversaw them gradually gave way during the war to programs and policy makers of a different ilk. The programs devised by these policy makers were at once narrower and more conservative than earlier programs. Moreover, they were predicated, by and large, on the simplistic notion that the South's future rested on its ability to create a business climate sufficiently favorable to attract industry—high-tech industry, hopefully, but low-tech or even no-tech industry if need be.

The federal government would aid this effort in a variety of ways over the years, but two types of programs would prove particularly important: those that channeled funds into the South to support improvements in the region's infrastructure, and those that facilitated the incorporation of the region into the U.S. military-industrial complex. Such programs played into the niggardly hands of "Whiggish" political and business leaders in the South who, by appearing "moderate" on the race question and "modern" in vision, have since the 1950s generally been able both to control federal monies coming into the region and to determine the limits of southern economic development. The geopolitical configuration we call the Sunbelt, a two-tiered society of "haves" and "have nots" divided largely along racial lines, was the result of this parlous and cynical strategy.

This is a work with many strengths. It is well researched and well argued, and the author skillfully employs tools and insights from several disciplines in his compelling, if not altogether convincing, analysis of the rise of the Sunbelt South. To my mind, Schulman underestimates certain sources of economic dynamism in the South in the period between 1880 and 1930, exaggerates to a degree the importance and certainly the coherence of federal policy toward the South in the period from 1938 to 1980, and is overly harsh on the so-called Sunbelt Whigs. But these are matters of emphasis open to individual interpretation; that Schulman has made an important contribution to the historiography on the modern South there can be no doubt.

PETER A. COCLANIS
University of North Carolina, Chapel Hill

ELIZABETH FAUE. *Community of Suffering and Struggle: Women, Men, and the Labor Movement in Minneapolis, 1915–1945*. (Gender and American Culture.) Chapel Hill: University of North Carolina Press. 1991. Pp. xviii, 295. Cloth $39.95, paper $14.95.

In writing about the labor movement in twentieth-century America, historians have told a story primarily of class and secondarily of ethnicity and race. Gender has only been a major factor in works dealing with wartime, when women assumed men's jobs, and in studies of industries where female labor prevailed, such as the garment trades. In her important book on the labor movement in Minneapolis, Elizabeth Faue has taken the familiar picture of labor militancy at the end of World War I and again during the Great Depression and World War II and refocused it through a gendered lens. Not only does she show how the 1930s emerged as a critical period of gender reorientation in the labor movement but, even more significantly, she also exposes the gender implications of much that transpired in workers' struggles to unionize from 1915 to 1945.

Faue argues that in the mid–1930s the labor move-

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ment in Minneapolis shifted from being community based and hence inclusive of women to being more workplace oriented, more bureaucratically and nationally centralized, and hence more marginalizing of women. She traces this transformation from a strike of telephone operators in 1918, which she claims celebrated community solidarity, to political actions of the early 1930s—including the famed truckers' strike of 1934 and Farmer-Labor Party activism—which called on community-wide mobilization to labor struggles in the late 1930s and the early 1940s where women despite, and often because of, their growing numbers in the work force encountered national unions that excluded them from membership, leadership, and the larger labor movement culture. Along the way, numerous familiar events take on new meaning; for example, the government's shift from granting relief through the Federal Emergency Relief Administration to providing jobs through the Works Projects Administration, and organizers' reorientation from mobilizing the unemployed in their neighborhoods to appealing to workers through the CIO, both contributed to the decline of a grassroots-family orientation and to the rise of a male-workplace orientation. In an imaginative part of the book, Faue traces the increasing masculinization of the iconography of labor within the union press over the course of the 1930s. In the symbolic discourse of the labor movement, assertive (even violent) male images represented union solidarity, while female imagery emerged only to depict victimization and to portray want and need.

As logical and convincing as Faue's argument seems, she sometimes has trouble sustaining it. To some extent this problem can be attributed to the complex and contradictory ways that change often takes place, but the author should better explain how the detailed descriptions of strikes that she presents demonstrate the larger analysis that she is making. Nor is the transformation as convincing as it might be because she fails to show the extent to which the telephone operators' strike of 1918 or the iconography of the labor press before the mid–1930s offered a community-based, familial alternative. But these concerns aside, Faue accomplishes just what a gender analysis at its best should do: to demonstrate how paying attention to gender can put an entirely new cast on a story we thought we knew well.

LIZABETH COHEN
Carnegie Mellon University

SARA M. EVANS and BARBARA J. NELSON. *Wage Justice: Comparable Worth and the Paradox of Technocratic Reform*. (Women in Culture and Society.) Chicago: University of Chicago Press. 1989. Pp. xiv, 224. $24.95.

Women workers earn substantially less than do men, and this earnings gap has persisted for a long time. The failure of the gap to narrow, even with changes in the legal environment that came with equal pay and civil rights laws, led to a new analysis of the problem and to a new policy prescription. The new analysis focused on sex segregation of the occupational structure and noted that the vast majority of women are employed in a small number of low-paid, female-dominated occupations. Many frustrated observers have concluded that the earnings gap will not narrow for a long time if we wait for large numbers of women to find their way into the higher paid men's jobs. Therefore attention has turned to improving the pay of women in female-dominated jobs and specifically to the policy of comparable worth.

Comparable worth is defined as the principle that jobs dissimilar in nature can be compared in terms of knowledge, skill, effort, responsibilities, and working conditions, and that jobs equivalent in value in these terms should be paid equally. Sara M. Evans and Barbara J. Nelson have investigated many aspects of comparable worth, including its historical background, the political controversy surrounding it, its economic rationale and results, the practical issues of implementation, and above all how it was implemented in Minnesota in the 1980s. They perform a service to the reader by summarizing much of the expert testimony on comparable worth given to the U.S. Civil Rights Commission, but they engage in simplistic characterizations of the positions of advocates and opponents. Although readers of the *AHR* may be most interested in chapter 2, where the authors briefly review the history of the earnings gap in the United States, what makes this book unique and fascinating is the Minnesota story.

Evans and Nelson offer an insider's perspective on the story of the passage of comparable worth legislation and its implementation at state and local levels in Minnesota. They were there when it happened and talked to the people involved. They take us through the nitty-gritty of the job-evaluation scheme on which comparable worth adjustments must be based. They show us political intrigue, obstructionist officials, and the worries of professional and male employees who believed that their position in the salary hierarchy was threatened by comparable worth. There are villains and heroines aplenty, and a few heroes. It is a good story with an appropriately ambiguous ending. Although comparable worth in Minnesota did succeed in raising the wages of many low-paid women workers, the authors are troubled by the fact that the policy was implemented by managers using modern personnel management techniques (the technocratic reform of their title). Workers had little to say about how the reform was structured or implemented; indeed, many beneficiaries were unaware that their wages had increased because of comparable worth. The authors see this failure to increase workers' power as paradoxical and therefore conclude by giving comparable worth a qualified passing grade: employees, mostly women at the lowest ranks, re-

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ceived more money; managers in most cases emerged with more power (p. 18).

ELYCE J. ROTELLA
Indiana University, Bloomington

DAVID E. HAMILTON. *From New Day to New Deal: American Farm Policy from Hoover to Roosevelt, 1928–1933*. Chapel Hill: University of North Carolina Press. 1991. Pp. xi, 333. $39.95.

David E. Hamilton has written what will be the standard study of Herbert Hoover's agricultural policy, and he offers insights into the development of Franklin Roosevelt's agricultural policy as well. Hamilton demonstrates that the Federal Farm Board and other Hoover farm programs were not just palliatives. Following Ellis W. Hawley, Hamilton argues that Hoover's agricultural programs reflected Hoover's larger vision of a national economy based on associations. Hoover actually hurt his popularity with farmers by refusing to sacrifice his long-term goals for immediate political gains. Hamilton's discussion of the origins of the Agricultural Adjustment Administration (AAA) reaffirms the crucial role of M. L. Wilson and other agricultural economists. Among them, he suggests, Mordecai Ezekiel deserves more credit, and John D. Black less than other historians have given.

Hamilton analyzes Hoover's associationalism and its rivals in ideological terms. This approach allows him to convey the extent to which Hoover was both anticompetitive and antistatist. But Hamilton's conception of policies as expressions of ideologies obscures the extent to which policies also express interests. Even Hoover's devotion to high industrial tariffs is explained by his rigid adherence to his beliefs rather than by the dominance of eastern industry and finance within the Republican Party.

Hamilton asserts a continuity between Hoover's Farm Board and Roosevelt's AAA, both of which he classifies as "associational." He contrasts these programs with "counterorganizational" alternatives such as McNary-Haugenism and inflationism. For Hamilton, "the great irony of the 'watershed of 1933' is not how often the New Dealers abandoned Hooverian goals, but how much better they were at fulfilling them" (p. 242), because they were so much less rigid than Hoover had been. Hamilton explicitly takes associationalism more seriously as a component of New Deal farm policy than other historians have done. In emphasizing associational ideology as the link between the Farm Board and the AAA, however, he takes too much emphasis away from production control, which contemporary political actors, including Hoover, Roosevelt, and the farm groups, saw as central to the AAA, and which made that program such a break with previous farm policies.

As presented by Hamilton, Hoover was neither the heartless do-nothing of liberal mythology nor the champion of individualism he proclaimed himself to be during the 1932 campaign and throughout his long ex-presidency. Hamilton is sympathetic to Hoover's purposes yet critical of his vision and its implementation. Hoover recognized farmers' need for organization, but not their need for foreign markets. Hoover also failed to understand the impact of technological change on farmers or why it was so difficult to mobilize farmers for collective action. In Hamilton's view, the Farm Board, the center of Hoover's agricultural policy, would have failed even without the devastating effects of the Great Depression, which began shortly after the Farm Board's creation.

Hoover's version of associationalism disappeared from agricultural policy debate after the election of 1932. Hamilton's findings suggest that not much was lost.

KENNETH FINEGOLD
Rutgers University, New Brunswick

SEAN J. SAVAGE. *Roosevelt: The Party Leader, 1932–1945*. Lexington: University Press of Kentucky. 1991. Pp. vii, 232. $25.00.

Sean J. Savage, a political scientist, has written what he says is the first book to focus exclusively on Franklin D. Roosevelt's role as party leader, covering the period from the mid–1920s through the election of 1944. The book's central theme is that FDR had a clear aim throughout this period: to make the Democratic Party a liberal party, for reasons both of personal ideological commitment and of belief that this was the key to Democratic success at the polls.

The book is organized topically, dealing first with the development of what Savage calls Roosevelt's "party-building strategy" in the 1920s and during the New Deal, and then covering FDR's political relationships with the city bosses, the Democratic National Committee, the elections of the 1930s, the 1938 purge, and the war years. Savage's focus on these topics is not always constant, leading to some lack of clarity; moreover, there is insufficient attention to chronology, with some resultant confusion as to development over time.

It is not easy to separate party leadership from other aspects of FDR's political career, and indeed Savage does tend to wander back and forth from his stated focus, both on FDR personally and on the question of party leadership. Overall, the book's content is not greatly different from that of other studies of Roosevelt as politician, and its addition to what we have learned from James MacGregor Burns and others is relatively modest. This is true in terms of specifics as well as generalities; his chapter on the urban machines, for example, differs little from earlier interpretations by Lyle Dorsett and others.

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The chapter on FDR and the Democratic National Committee is the most original and is a welcome addition to our knowledge of Democratic Party development during the New Deal years. Savage also makes a contribution to the continuing debate about the role of liberal ideology in explaining Roosevelt's political behavior. Savage's main argument that it was central, was seen by Roosevelt as proper in terms both of principle and of practical politics, and was consistent from the mid–1920s to the end of Roosevelt's career, is worthy of note.

But the book does not hold together very well. It reads more like a series of articles on Roosevelt than a unified monograph. It lacks a persuasive organization as well as constant focus on FDR as party leader. It is well researched and documented, grounded in extensive study of the relevant primary materials. The writing is inelegant and occasionally faulty, but quite comprehensible. A bibliography would have been helpful, and the publisher should be faulted for the clumsy economy of grouping the endnotes into paragraphs of five notes, making it unnecessarily difficult to find a citation.

The book does not make a major contribution to our understanding of Roosevelt's politics. But it is useful, both in terms of its content and its interpretation. Specialists in the study of Roosevelt and the Democratic Party will find it worthwhile.

JOHN M. ALLSWANG
California State University, Los Angeles

WILLIAM GRAEBNER. *The Age of Doubt: American Thought and Culture in the 1940s.* (Twayne's American Thought and Culture Series.) Boston: Twayne. 1991. Pp. xv, 192.

The 1940s is a notoriously difficult decade to write about. Unlike the 1920s or the 1930s, which each have a clear beginning and end conveniently characterized by prosperity or depression, the 1940s rarely appears to be a unified entity at all. The first half of the decade is taken up, of course, by World War II —which, whatever its substantial legacy for the postwar years, remains a distinctive experience abroad and at home. The five years following 1945, dominated by the Cold War, the onset of McCarthyism, and the return of affluence, really seem a preparation for, if not an intrinsic part of, the 1950s. Thus, anyone seeking to explore the "1940s" as a singular episode in modern American history has the special burden of persuading us that there were unique and continuous tensions, attitudes, trends, and ideas that set the decade apart from what came before and after. This may be one reason why so few historians or literary critics have attempted to study the decade as a whole.

William Graebner has tried, imaginatively though not entirely successfully, to bring some coherence to the period. His central theme is the disparity between the surface enthusiasms associated with the wartime triumph and the postwar economic boom, and the underlying feelings of doubt, skepticism, anxiety, and insecurity that nevertheless afflicted many Americans throughout these years. On the one hand, he argues, people had every reason to be confident about the country's military power, political stability, and technological supremacy. On the other hand, the atomic bomb, the Holocaust, the growing conflict with the Soviet Union, the fear of internal subversion, and the difficulties of readjusting in peacetime to home and family all reminded Americans that they were living in a time of chance and uncertainty when a stable system of values to guide the conduct of nations and individuals no longer existed. And, to make matters worse, no one emerged to offer political or philosophical relief. Instead, Americans were taught that most problems were ultimately insoluble, that human life was marked by perpetual crises and tragedies, and that they should cultivate a sense of irony and paradox as a form of emotional survival.

Graebner's portrait of the 1940s is not necessarily new. But he moves beyond the more traditional realms of political rhetoric and intellectual pronouncements to examine the ways America's contradictory feelings were reflected in fads, fashion, and mass culture. Graebner is especially skillful in analyzing best-selling psychology and self-help books, developments in modern painting and architecture, popular music, and fiction. His favorite source, however, is Hollywood. No matter what the subject, Graebner invariably turns to the movies—comedies, musicals, film noir, topical dramas—to reinforce his interpretation. His treatment of particular films is sometimes inflated, a common trait among media scholars who take the cinema so seriously that they forget how much they themselves (like everyone else) simply loved going to the movies. Still, Graebner is often ingenious in his assessments of plots, performances, and the conventions of 1940s filmmaking.

This is a brief but extremely valuable book, largely because of its interdisciplinary perspective. It may not change our customary way of looking at the 1940s, but it is filled with insights and ideas that certainly deserve our attention.

RICHARD PELLS
University of Texas, Austin

GILBERT C. FITE. *Richard B. Russell, Jr., Senator from Georgia.* (The Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1991. Pp. xiv, 566. $29.95.

Gilbert C. Fite, a leading agricultural historian, has written a skillful and dispassionate biography of Georgia Senator Richard B. Russell, Jr., who chaired the Armed Services Committee during the Cold War.

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The book is based on exhaustive research in the Russell archives at the University of Georgia and provides a detailed examination of Russell's life and career. Fite's plausible central argument is that Russell willingly sacrificed his presidential aspirations on the altar of white supremacy.

In his political beliefs, rise to power, and mastery of parliamentary procedure, Russell typified conservative southern Democratic congressmen in the twentieth century. Born in 1897 to a privileged Georgia family, Russell faced constant paternal pressure to succeed in politics. Dutifully, Russell cultivated personal contacts at the state university, gravitated to political power centers, shunned the limelight and controversial issues, and usually made his word his bond. He never married, but had a mistress. After short stints as a state legislator and governor, the thirty-six-year-old Russell reached the U.S. Senate, where he remained until his death thirty-eight years later in 1971. By then, Russell was president pro tem of the Senate and suffering from hypochondria and emphysema.

Although Russell did not write his name into any major legislation, he did exert powerful influence in Congress. Most important, he shepherded through the Senate enormous military expenditures to defend against the Soviet menace. As the second-ranking member on Appropriations, he called for generous farm subsidies and sponsored legislation to create the school lunch program. He also orchestrated the southern Democratic bloc to defeat or weaken hated civil rights bills. In all of this congressional maneuvering, Fite pays insufficient attention to the House of Representatives' critical role in the legislative process. The pinnacle of Russell's career came in 1951, when he defused widespread unhappiness over President Harry Truman's firing of General Douglas MacArthur for insubordination during the Korean War.

The great irony of Russell's public life involved his ambitious protégé, Lyndon B. Johnson. After Russell's 1952 Democratic presidential bid soured, Russell boosted Johnson to Senate leadership in the hope that a more moderate Johnson could appeal to northerners and still preserve segregation. When Johnson later became president, Russell sagely counseled him to avoid war in Vietnam. Johnson did neither. And yet, Russell endorsed—reluctantly, to be sure—Johnson's reelection in 1964 and his tragic foreign policy.

Russell is portrayed here as a statesman who avoided political opportunism. Repeatedly, however, Russell held his forefinger to the winds of public opinion, allied himself with utility and fertilizer companies, shelved friendship when it suited his purposes, and threw campaign mudballs when they came his way. What Russell, the self-proclaimed fiscal conservative, excelled at was extracting pork barrel defense plums for his state and supporting nearly every Pentagon request. This is not necessarily to indict Russell, but merely to recognize that he was a political animal.

Fite deserves credit for point-blank labeling Russell a racist, for there can be no question of his obsession to preserve Anglo-Saxon society. In the bitter Senate campaign of 1936, Russell assured his supporters that "this is a white man's country, yes, and we are going to keep it that way" (p. 145). He refused to meet with black constituents and vigorously resisted legislation that would end lynching and guarantee equal access to public accommodations, government jobs, and the polling booth. Angered by northern liberals, Russell suggested that, since they loved blacks so much, the federal government pay $1,500 to each southern black family relocating in the North. Fite then inexplicably defends Russell by concluding that the Georgia senator "did not wish ill for blacks" (p. 165). This confuses Russell's general gentility with his racist objectives. Russell may not have been, as Fite alleges, another demagogue like his Senate colleague Theodore Bilbo of Mississippi, but Russell's actions cut blacks just as badly.

These criticisms aside, this old-fashioned biography fills a significant gap in the historical literature. Southern Democrats who for so long held the reins of congressional power are now getting some of their due. Historians have recently chronicled the careers of such twentieth-century southern stalwarts as Oscar Underwood, Sam Rayburn, Pat Harrison, J. William Fulbright, Sam Ervin, and Howard W. Smith. At last, another of these congressional powerhouses, the "Georgia giant" (p. xiii), has been given his due in Fite's definitive study of Richard Russell's life and times.

BRUCE J. DIERENFIELD
Canisius College

EDWARD D. BERKOWITZ. *America's Welfare State: From Roosevelt to Reagan.* (The American Moment.) Baltimore: Johns Hopkins University Press. 1991. Pp. xxii, 216. Cloth $38.95, paper $11.95.

Edward D. Berkowitz's volume is somewhat narrower in scope than the title indicates. His purpose is to present a historical account of the creation and subsequent development of the Social Security Act of 1935. That legislation not only included the insurance program popularly called "Social Security" but also included unemployment compensation and relief programs for certain groups, specifically the elderly, the blind, and dependent children. This book does not cover other welfare programs, such as public housing, food stamps, or temporary work relief.

The author's major contribution is his focus on policy changes and program proposals since the passage of the original legislation. For example, he spends twelve pages on Richard Nixon's abortive Family Assistance Plan. Berkowitz also deliberately emphasizes the perspective of bureaucratic policy

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makers as opposed to outside pressure groups and program recipients. In order to highlight today's concerns with the fiscal soundness of Social Security, the size of our welfare rolls, and the large number of people with no health care coverage, Berkowitz has organized his material into three parts: Social Security, welfare, and national health insurance. Thus, the interaction among the three areas is somewhat disrupted. To tie the topics together, the author uses the theme of conflict in launching these programs in the 1930s and 1940s, consensus (by which he means widespread, optimistic support for these programs) in the 1950s and 1960s, then back to conflict in the 1970s and 1980s as confidence in the soundness of our welfare structure faded. This theme tends to downplay the extreme sense of crisis that made possible the revolution in the United States in our welfare system that occurred in 1935. Instead, Berkowitz stresses how conditions have changed. For example, given unemployment that hit 25 percent, it made sense to pay mothers in the Great Depression to stay home. By the 1960s, working mothers were common; and the newly renamed Aid to Families with Dependent Children program was amended to coerce mothers on welfare into job training.

This book has several drawbacks. Some important topics are omitted. The discussion of the failure to include national health insurance in the 1935 legislation leaves out the fact that Crippled Children's Services (important in an era of no vaccine for polio) did make it into the 1935 law. Also, the author goes through a fairly detailed analysis of the 1950 Social Security amendments, which expanded coverage and raised benefit levels, without once mentioning Harry Truman. In addition, the book suffers from a lack of footnotes. In their place, Berkowitz has included brief bibliographic essays for each chapter, but these fail to provide much guidance for researchers who may wish to locate a specific source.

This book is clearly written and enlivened with some of the more colorful cases used by the bureaucrats to make their points. While its emphasis on the compromises of these policy makers is a useful addition to the literature on Social Security, it does miss some of the drama inherent in the passage and development of what is probably our most important piece of domestic legislation.

Judith Ann Trolander
University of Minnesota, Duluth

Rip Bulkeley. *The Sputniks Crisis and Early United States Space Policy: A Critique of the Historiography of Space*. Bloomington: Indiana University Press. 1991. Pp. xvi, 286. $29.95.

This is an interesting but curious little book written by a self-styled "peace researcher" desirous of learning Americans' "underlying attitudes toward space policy" (p. viii) as formed by the shock of the first sputniks in 1957. One might expect a tendentious account berating the Cold War militarism of the United States, and indeed Rip Bulkeley argues that the International Geophysical Year (IGY) and Vanguard satellite project were not innocent scientific undertakings, but promotions aimed at boosting American prestige vis-à-vis the Soviet Union. But rather than damning the Dwight Eisenhower administration for perverting space technology with Cold War concerns (it could hardly have done otherwise), the author concludes that the United States lost the race in space because it did not, early on, prosecute the Cold War vigorously enough. In particular, he affirms the claim made by some Eisenhower defenders at the time to the effect that the "lost years" under Harry Truman were the real reason why the Soviets were able to grab a lead in missiles and space.

But if Eisenhower and his advisers were not insouciant toward "this race that we cannot afford to lose" (as Nelson Rockefeller stated in a National Security Council [NSC] document), then why did the United States come in second? That is the central mystery of early space history. The author's documents reveal that American estimates of Soviet progress in rocketry were actually more accurate during the first years after World War II than after 1950, when they tended to belittle Soviet potential for rapid progress. In light of the Soviet atomic explosion of 1949 and the U.S. military build-up inspired by NSC 68, this trend in intelligence estimates seems counterintuitive. Bulkeley exposes the riddle, but does not solve it. Likewise, he traces in detail the American role in planning the IGY and its satellite programs, but falls short of a persuasive explanation for why the Stewart Committee, charged with selecting the means for launching a satellite, opted for the small "start-from-scratch" Vanguard rather than Werner von Braun's army plan that could have produced satellites months earlier. His provocative theory is that the scientists themselves were anxious to secure "a substantial say in the formulation and implementation of non-scientific national policy decisions affecting their own work" and to ensure "that genuine scientific achievement should become, as never before, a major dimension in the domestic pride and foreign prestige of the United States" (p. 177). Hence, the Stewart Committee's "elitist agenda" demanded a minimization of the military's role in the space program and, as a result, Eisenhower was a victim of bad, self-interested advice. Perhaps so—the maneuvering of scientists during the great build-up in federal research and development cries out for full historical treatment—but it is hard to accept the author's conclusion concerning the IGY/Vanguard decisions in the absence of a fuller account of the strategic considerations involved in the satellite decision, particularly concerning the Air Force project to develop spy satellites.

In my own book, *. . . the Heavens and the Earth* (1985), I suggested that the Defense Department

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purposely used the IGY and an innocuous, nonmilitary sounding rocket in order to increase chances for world acceptance of the legality of satellite overflight in anticipation of the spy satellites to come. But Bulkeley rightly notes the paucity of hard evidence for this theory (although his own research produces what, to my mind, is more circumstantial evidence for it). So the mystery surrounding the Soviet victory in the first "space race" will remain until all the documents still blacked out come to light. Perhaps, now that the Cold War is over, we will not have to wait long.

One defect in this book that begs mention, because it is so obtrusive, is its disparagement of all previous writers on the subject. The author constantly uses phrases such as "historians have often commented," "the space historians of the 1960s," "even the best space historians," "few American space writers or historians," "yet to convince the space historians," "historians have frequently stated," and so on. The offenders are not always identified, and when they are they often turn out to be political writers, journalists, memoirists, and popularizers from the 1950s and 1960s, or else NASA historians whose excellent work is dismissed for being "in-house." To be sure, the author is right to lament the fact that skewed accounts of the sputnik crisis, born of Lyndon Johnson's disingenuous Preparedness Committee hearings, are still in circulation. But there is no need to debunk old Ike-bashing canards, over and over again, to establish one's own revisionist credentials. Document-based historiography of space technology is thriving, as this book proves, and the importance of the questions raised, if not fully answered, by Bulkeley ensures that it will continue.

WALTER A. MCDOUGALL
University of Pennsylvania

MARC TRACHTENBERG. *History and Strategy*. (Princeton Studies in International History and Politics.) Princeton: Princeton University Press. 1991. Pp. xii, 292. Cloth $44.50, paper $14.95.

Marc Trachtenberg has written a work of incisive historical scholarship and an appeal for greater interaction between historians and scholars in other disciplines concerned with the study of war and peace, particularly political scientists. He chides scholars in other fields for their failure to appreciate fully the value of historical analysis in understanding nuclear strategy in the post–World War II era. He provides several examples that clearly show that a sense of historical continuity is an essential guide to the study of major nuclear issues and episodes. Trachtenberg also suggests that historians can be misled by their own predispositions, their methodology, or the sheer volume of their sources, and that they can benefit greatly from the insights of political scientists.

Those points might appear so obvious that they hardly bear repeating, but Trachtenberg gives them fresh vitality by demonstrating that a lack of cross-disciplinary scholarship has seriously impeded understanding of important diplomatic and military questions in the nuclear age. He develops his argument in a series of discrete essays that offer fascinating and arresting new perspectives. In a discussion that makes a notable contribution to the well-worn topic of the evolution of nuclear strategy, for example, he remarks on the lack of historical consciousness on the part of the leading practitioners in the field. Trachtenberg finds this to be a fundamental, and by the mid–1960s, fatal flaw in their approach to nuclear issues.

Perhaps the best part of the book is a lengthy essay on the Berlin crises of 1958–62. Trachtenberg argues that this subject has received too little attention from scholars and cites it as the key to maintaining peace in Europe after the early 1960s. He takes issue with those who have viewed the existence of nuclear arms in Europe as the explanation for its political stability. He shows that the reasons are much more complicated and traces the shift in U.S. and Soviet positions over time. The result, he contends, was that both nations reached a point where they were willing to accept the status quo and unwilling to risk a nuclear war over Berlin. The Berlin crises, he concludes, "might have led to war" but instead "served to clear the air" (p. 234).

Trachtenberg's book also includes first-rate essays on the origins of World War I, the shift in American nuclear strategy between 1949 and 1954, the nuclearization of NATO, and the Cuban missile crisis. Throughout the volume he calls for detailed historical analysis to help guard against the misuse or misapplication of historical examples. The book is so rich in insight that all historians of the nuclear age should consult it. Political scientists and scholars in other disciplines who study nuclear strategy and international politics would profit from heeding Trachtenberg's advice that they develop what he calls "historical sensibility" (p. 262). This volume is a good place for them to begin.

J. SAMUEL WALKER
U.S. Nuclear Regulatory Commission

THOMAS ALAN SCHWARTZ. *America's Germany: John J. McCloy and the Federal Republic of Germany*. Cambridge: Harvard University Press. 1991. Pp. xiii, 404. $29.95.

This fine diplomatic biography of John J. McCloy is also a history of German-American relations during a crucial, formative period of the Cold War. Thomas Alan Schwartz argues that McCloy and other American policy makers pursued a "dual containment" strategy that was intended to resolve the German problem by fully integrating the Federal Republic into the West and at the same time to stabilize a

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balance of power in Europe by making use of West Germany's considerable military potential. Although his thesis is not new, Schwartz brings a wealth of detail to this complex story, thanks to his extensive employment of American, German, British, and, to a lesser degree, French sources.

After describing McCloy's early career in law and government and summarizing American policy toward Germany prior to 1949, Schwartz gets to the marrow of his topic: the formation of the Federal Republic and its surprisingly rapid transformation from puppet to alliance partner. In the early going the issues were mainly economic, although high politics always lurked in the background. Schwartz is particularly adept in tracing the origins and significance of the Schuman Plan, that "government-sanctioned supercartel" (p. 103) and centerpiece of Franco-German reconciliation without which the policy of integration could not have succeeded. Schwartz also delves beneath the surface of diplomacy by investigating the influence of industrial interests on these negotiations.

The major turn in policy came with the Korean War and the recognition in the United States that an effective defense of Western Europe would require German economic and military power. Schwartz skillfully traces this story through all its ups and downs, beginning with American and French ideas for a European army and ending with the acceptance of the Bundesrepublik into NATO in 1955. In the process, he demonstrates that the ill-fated European Defense Community (EDC) was more than a mischievous French proposal. Its extended tryout was prolonged by American managers who, impatient with a piecemeal approach, saw in the EDC an institutional device for resolving all their problems in one swing.

Personalities are important in this work—not only McCloy but also Jean Monnet and particularly Konrad Adenauer. Schwartz points out how dependent U.S. policy became on Adenauer's continued mastery of the delicately balanced internal politics of the Federal Republic. Although in retrospect the Federal Republic's integration into the West looks inevitable, without Adenauer's statesmanship and vision the postwar story might have been far different.

Although Schwartz is critical of America's failure to mete out full punishment to convicted war criminals, his is a sympathetic account written from a mainstream point of view. Of course, the once-standard complaint that U.S. policy delayed reunification and prolonged the Cold War looks somewhat lame in the light of recent events. But even from an establishment perspective dual containment did have some serious drawbacks. As U.S. policy makers of the 1960s came to recognize, they had mortgaged their policy to a presumably nationalist German public opinion on the reunification question, which led to a dangerous inflexibility on Berlin and other Cold War issues. Given its importance, Schwartz might have examined at greater length the American perception of German national character and its relation to Cold War policy.

Inevitably, when dealing with a topic of this complexity, problems of scale and focus are bound to arise. To his credit, Schwartz is aware of flaws that appeared on the (equally important) Soviet side of this twin-hulled containment policy, but grappling with them would have meant ranging too far afield. Moreover, the book's ambition of being both a biography and a full-dress treatment of German-American relations results at times in McCloy's getting lost in the larger drama. But on balance the author's decision to cast a wider net is to be applauded.

The impressive virtues of this topflight work of diplomatic history far outweigh any complaints one might make. It is the best available work on the topic and indispensable reading for all students of the Cold War.

FRANK NINKOVICH
St. John's University
Jamaica, New York

REINHOLD WAGNLEITNER. *Coca-Colonisation und Kalter Krieg: Die Kulturmission der USA in Österreich nach dem Zweiten Weltkrieg*. (Österreichische Texte zur Gesellschaftskritik, number 52.) Vienna: Verlag für Gesellschaftskritik. 1991. Pp. 435.

In a postscript to his last chapter, Reinhold Wagnleitner quotes E. H. Carr: "Study the historian before you study the facts." No better advice could be given to the readers of this work. Is it important to know that the author worked for a dozen years as a bass player for jazz, rock, and pop groups? Is it relevant that the author is clearly a member of the current critical school of social research and that the book under review is part of the series *Österreichische Texte zur Gesellschaftskritik*? Of course it is, with the caveat that a dozen years spent as a jazz performer is not really disabling, but serves to give the author a genuinely comprehensive perspective on popular culture, which, after all, is the focus of this work.

Although the principal concern of the study is the United States' "cultural mission" in Austria after World War II, Wagnleitner has really presented us with a comprehensive analysis of the major dimensions of modern culture, both high and popular. By doing so he lays a solid foundation for an examination of U.S. activity in Austria and the manner in which this activity was received by the target population. Another noteworthy quality of the work is the encyclopedic reach of the author's bibliography and, more importantly, his genuine command of this vast literature. The one difficulty is the vacuum cleaner technique that sucks up and processes every tidbit it is able to ferret out. In a way, everything is grist to the author's "critical" mill and thus whatever "American" funneled abroad is, ipso facto, a conscious instrument for establishing and cementing U.S. political hege-

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mony. Again, one must concede the methodology of the critical study of popular (as well as high) culture, but readers coming from other "mansions" in this great house of history (or social history) may be forgiven for not being fully convinced of this "big bad wolf" dimension of the author's remarkable skill and diligence in mustering his arguments.

The most fascinating dimension of this study is Wagnleitner's absolute command of the documentation concerning U.S. cultural activities in Austria immediately after the war and in the emerging Cold War period. His grasp of the intricacies of making and administering this cultural policy is first-rate, extending even to a solid understanding of the possible friction between Congress and the executive branch (including the military) over cultural policy implementation. Conservative members of Congress might well have been appalled about the United States helping to spread jazz/pop/rock music; but the people executing the policy on the spot were only too well aware of the tremendous popularity of that music among young Austrians. At the same time, spreading American high culture was much more difficult, for the target audiences were firmly convinced that American high culture had nothing to offer them.

Nearly a fourth of the volume is devoted to laying out the pre–1945 foundations. After that, successive chapters deal with U.S. policy regarding the press, radio, literature, education, drama and music, and Hollywood: the title of a subsection in the chapter on Hollywood will do more to suggest the main thrust of the book than a long explanation: "Der Hollywood Film als Text der Macht." Those who are hostile to the "critical school" have ample opportunity for criticism. One could be either amused or angered by the suggestion that the CIA would surely not have financed a European exhibit of American abstract impressionism if it had not been seen as an effective counter force to the socially engaged realism in art of the New Deal period (p. 353). I choose to be amused and intrigued; others might not share this perspective.

There are sixteen pages of illustrations, forty-four pages of notes, and a bibliography of thirty-four pages. Is it an unreasonable complaint to say that an index would have been very helpful?

ALFRED DIAMANT
Indiana University, Bloomington

IRWIN M. WALL. *The United States and the Making of Postwar France, 1945–1954*. New York: Cambridge University Press. 1991. Pp. x, 324. $47.50.

Irwin M. Wall has written a most welcome book on Franco-American relations from 1945 to 1954. His argument is that the war and its consequences produced an unnatural dependency. France desperately needed grain, fuel, and dollars. Consequently, the United States was able to exert enormous influence on French diplomatic, political, economic, and cultural affairs. The French carped and remonstrated. Gradually, French nationalism grew and asserted itself. Pierre Mendes France represented the French quest for autonomy and independence. In 1954 he rejected the European Defense Community (EDC) and withdrew French troops from Indochina, thereby defying the wishes of the Eisenhower administration and setting patterns that would characterize the strained relationship between Charles de Gaulle and the United States after 1958. The United States wanted a centrist France amenable to American influence but wound up with a France dominated by Gaullism on the right and a "left allied with Communism." According to Wall, "The Americans were never able to create the suitable France for which they searched" (p. 298). American influence was pervasive yet ineffective.

Wall makes excellent use of French foreign ministry records and manuscript collections as well as State Department decimal files and some Economic Cooperation Administration (ECA) papers to illuminate such matters as U.S. dealings with the French Communist Party and the French labor movement, the allocation of Marshall Plan counterpart funds, the bickering over levels of military aid and rearmament expenditures, and the acrimony about French policy in Indochina. According to Wall, his aim is not to highlight American influence but to elucidate the constraints under which it operated. Americans were effective only when they worked with elites and bureaucrats who shared their own goals. These elites had their own reasons for pursuing anticommunist policies and were able to manipulate American efforts to suit their own purposes. Indeed, French weakness afforded these elites considerable leverage because U.S. officials were always afraid that if they pushed too hard they might play into the hands of the Gaullists or, worse yet, the Communists. For example, Wall argues that "traditional elites" (for the most part undefined) were able to use the Marshall Plan "to maintain their hold on power" (p. 73). And even when the United States sought to mold the shape of the French labor movement, the Americans enjoyed only fleeting success. "French labor leaders took Washington's help, but never its orders, and the structure of the French labor movement remained shaped by indigenous historical traditions" (p. 4).

Given that Wall takes American influence "for granted" (p. 9), it is not surprising that he minimizes its significance. Yet even within the confines of his own analysis, Wall cannot quite ignore the impact that the United States had on French policy and European developments. American carrots and sticks persuaded the French to accept German industrial reconstruction and zonal union. The French did not want German rearmament and they buried the EDC, but they could not permanently stymie the build-up

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of German forces and their integration into NATO under one means or another. Furthermore, the American role in the formation of the Schuman Plan, Wall admits, "was critical at every stage of the process" (p. 194). And given the fact that the overriding goal of U.S. policy was anticommunism (p. 301), and given Wall's conclusion that the United States did do "so much" (p. 302) to accomplish the isolation of the Communists, it is difficult to see how the United States was quite as ineffective as he alleges. True, the United States did not want Mendes France or de Gaulle and did not restructure the balance of power within French society. But American officials, collaborating with European elites, did achieve their overall foreign policy objectives: they contained the Soviet Union, eroded the influence of indigenous Communists, and coopted Germany into an integrated Europe linked to an Atlantic community.

Differences over Wall's conclusions should not obscure the importance of the issues he raises and the methods he uses. He has written a nuanced study of a bilateral relationship and nicely illuminates the interdependence of Western elites and the convergence of their interests. He uses sources in both countries. He reminds us that weak nations are not without power, and he suggests that the diffusion of American culture, the culture of consumerism, may have been more important than its economic or political tutelage.

MELVYN P. LEFFLER
University of Virginia

BENNETT KOVRIG. *Of Walls and Bridges: The United States and Eastern Europe*. (A Twentieth Century Fund Book.) New York: New York University Press. 1991. Pp. xiii, 425. Cloth $32.00, paper $14.00.

Bennett Kovrig has written a masterful survey of American policy toward the Soviet satellite states in Eastern Europe since World War II. History, in the dramatic events of 1989, furnished him with the best possible stopping point. The book is more than just a sequel to his *Myth of Liberation*, published some twenty years ago, in which he correctly stressed the exaggerated hopes and overblown propaganda that often seemed to be a substitute for policy. Here, after a shortened run over the earlier years, he carries on through the 1970s and 1980s as Washington finally managed to find a reasonable balance between ultimate aims and practical policies.

While America could not do without slogans and catchwords, "bridge-building," "detente," and "differentiation" were more modest but also more apt than "liberation" when, faced with Soviet determination to hold fast, the need was for gradual and subtle erosion of the bloc, keeping the ultimate aim of freedom alive while not igniting a war or inciting nations to hopeless revolt. It is apparent from the record so carefully presented here that, with all the miscalculations, frustrations, and failures, U.S. policy over the period as a whole was well conceived and, within the obvious limitations, well carried out. Kovrig is generous in giving credit to the United States, over successive administrations, for steady purpose and flexibility in action. Generous, but not uncritical, as time and again in the course of his narrative he points out factors—the personal foibles of a president or secretary of state, the pressures of domestic politics, the failures of intelligence—that turned policies in this direction or that, not always in the interest of consistency or of practical success.

The full documentary record is not available to scholars, for which the tardiness of the State Department in getting out the annual volumes of *Foreign Relations of the United States*, and in opening the archives to the corresponding dates, is in large part responsible. The key volume covering the Hungarian events of 1956 came out only recently, well beyond a reasonable lapse of time and too late for use in preparing this study. Although it contains no striking revelations that would change Kovrig's narrative and conclusions, it would have provided a more solid documentary foundation. Much of the U.S. government's policy deliberations and diplomatic moves in recent years, with the press ever at the elbow of the secretary of state, become known almost immediately to the public; and if there are still secrets to be dug out, the Freedom of Information Act is there for those who choose to do the digging. Thus, when scholars after thirty years or more actually get into the archives, they may find the cupboard bare. Kovrig's account is soundly based and not likely to be overturned or even substantially modified by what comes to light later from official documents; his may serve as a standard work until, and even after, someone well on in the next century writes a "definitive" history.

The three episodes of crisis in 1956, 1968, and 1980–81 quite naturally deserve special interest and attention. Sharp questions have been raised. Was the U.S. government prepared for what happened? Could it have deterred or prevented the repressive action taken by Soviet armed forces in Hungary or in Czechoslovakia, or by the Polish military in 1981? Did it deceive or betray peoples who in their struggle for freedom put their faith in America, the self-proclaimed champion of freedom? They are simple questions which require complex answers or cannot be answered at all because of untested "ifs" of history. It must be said for Kovrig that he tackles them honestly, giving clear conclusions when the facts are there, and well-considered conclusions when they are not. In the Czechoslovakian affair of 1968, America's calculated indifference and professions of noninvolvement, intended to assure the Soviets that there was no cause for intervention, in effect told them they could intervene with impunity. Kovrig concludes that Washington "probably made a mistake" in not warning Moscow of the damage that Soviet armed inter-

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vention would do to the burgeoning superpower detente. That is putting it charitably.

Kovrig has no trouble with the dilemma historians often face in choosing a chronological or a topical approach. He adopts both, and although it means some duplication here and there, the advantages are manifest. The latter and larger part of the book takes up three major strands of American policy: human rights, economic levers, and security structures. As each of them had a life of its own and was pulled this way and that by the vagaries of domestic politics, the government's task, by no means an unmixed success, was to weave them together into consistent effective policy. The author performs a useful service in showing how the human rights issue, accorded relatively low priority before the Helsinki Conference in 1975, grew into a mighty force because it was in tune with ever-stronger currents within Eastern Europe itself as well as with the fundamental ideals of the West. Economic policy, however, described here in what amounts to a detailed history of East-West economic relations, produced relatively minor results, whether intended to induce or to coerce. Security, whether in the forefront or the background of official explanations, was always the controlling factor.

JOHN C. CAMPBELL
Council on Foreign Relations

JOHN PRADOS. *Keepers of the Keys: A History of the National Security Council from Truman to Bush.* New York: William Morrow. 1991. Pp. 632. $25.00.

The Iran-Contra affair revealed the enormous influence of the National Security Council (NSC) staff on American foreign policy in the Reagan years. In tracing the history of this new institution from its origins at the outset of the Cold War through the late 1980s, John Prados provides a clear understanding of how an agency designed simply to coordinate diplomatic and military policy became so all-powerful.

The author's central theme is the growing power of the national security adviser and his staff at the expense of the council itself and its statutory members—primarily the secretaries of state and defense. From a halting beginning under President Harry Truman, who did not even attend the earliest meetings, the NSC evolved into an important body under Dwight Eisenhower, who used it to delineate the important issues and examine alternative policies. Rather than the needlessly bureaucratic mechanism criticized by contemporaries, Ike's NSC was an effective body that the president relied on for advice, although never for final decisions, which he always made himself.

It was under presidents Kennedy and Johnson that the national security adviser, first McGeorge Bundy and then Walt Rostow, began to overshadow the NSC itself. Never an effective agency for crisis management, the council played little part in either the Cuban missile episode or the escalation of the Vietnam War. The growing power of the national security adviser and of the NSC staff as opposed to the council itself became most obvious in the 1970s when Henry Kissinger and Zbigniew Brzezinski used their positions to transfer policy making from the State Department to the White House. The culmination of this process came in the 1980s, when members of the NSC staff such as Oliver North, working under relatively weak national security advisers like Robert C. MacFarlane and John Poindexter, transformed the NSC into a covert operations center to mastermind the diversion of the profits from Iranian arms sales to fund the *contras* in Nicaragua in defiance of Congress.

Prados offers essentially an episodic history of the role of the NSC in the Cold War. He is strongest on the earlier period through the Johnson years, where he relies on the documentary record available in the presidential libraries. The second half of the book, based largely on memoirs, becomes more of a journalistic account, filled with personalities but lacking in sustained analysis and interpretation.

One conclusion flows clearly from Prados's history of the NSC: ultimate responsibility for its actions lies with the individual presidents. Leaders as different in style as Eisenhower and Kennedy manipulated the body so that it served their distinctive purposes. Richard Nixon and Jimmy Carter both allowed their ambitious national security advisers to conduct diplomacy through secret back channels, a process that undermined traditional State Department authority. And Reagan, by failing to articulate and supervise a coherent national security policy, allowed freewheeling NSC staff members to carry out what they assumed were his wishes.

Now that the Cold War is over, Prados's book should serve as a useful starting place as scholars search for a more precise understanding of the role of the NSC in the formulation and conduct of U.S. policy toward the Soviet Union. It offers a chronicle of the misuse of power in the name of national security and the dangers inherent in the growth of the imperial presidency.

ROBERT A. DIVINE
University of Texas, Austin

CANADA

KAREN ANDERSON. *Chain Her by One Foot: The Subjugation of Women in Seventeenth-Century New France.* New York: Routledge. 1991. Pp. 247. $35.00.

The often-told story of the Jesuits' missionary enterprise among the Huron and Montagnais during the first half of the seventeenth century is reexamined and dissected by Karen Anderson, then reassembled according to a feminist perspective. What this intriguing exercise establishes, according to Anderson, relying heavily on the *Jesuit Relations*, is that women were

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the main targets of the Jesuits in their evangelical campaign. Horrified at the equality and power enjoyed by women in the native societies, the French missionaries set about undermining the social institutions that allowed for such a situation. Their principal goal, as Anderson sees it, was the reorganization of those societies to put women in their proper place—one of subjugation. Huron and Montagnais women, once proud and independent, ended up being subjugated to men, who assured their victory by figuratively chaining them by one foot.

It is well established that at the time of first contact with Europeans, Amerindian women in general enjoyed greater freedom and power in their societies than did their European sisters overseas. They worked hard and life was far from easy for them, but gender roles were clearly defined and each controlled its proper sphere. What this meant in practice was that neither men nor women could do without each other; in fact, in Anderson's estimation, if anyone had an edge, it was the women. Although she claims to be analyzing both Huron and Montagnais societies in this regard, she focuses almost entirely on the Huron.

Anderson is at her best when dealing with the interplay of the sexes in Huron society, analyzing how access to the means of production and the labor of others was controlled through kinship and conjugal bonds. Men cleared the fields, women planted the crops. Men were the traders, hunters, and warriors; women tended the crops, harvested, gathered wild foods, and prepared and distributed the fare, whether hunted, grown, or gathered. In government, men were chiefs and councillors but women controlled the selection of candidates for those positions. Reciprocating duties and privileges meant that one side did not dominate the other, although Anderson contends that women were less dependent on men than men were on women.

To the seventeenth-century European, devoted to the notion of hierarchy, such a social order was seen as contrary to the laws of nature. The Jesuits, in attacking the Huron system, were acting from profound conviction, social as well as religious. To interpret this as an attack specifically on women, as Anderson does, is to take a selective view of what happened. And to claim that the Jesuits were successful in subjugating Huron and Montagnais women is to make too much of the consequences of conversion: the missionaries may have claimed that the women became "like lambs," but did that in fact occur in anything more than individual cases? If so, such meekness and subordination has not carried through the generations, as those familiar with Huron and Montagnais communities today can attest.

Anderson is on solid ground when she argues that subjugation is not the natural condition of women, but this work is too narrowly focused to sustain her point.

Olive Patricia Dickason
University of Alberta

Jean Barman. *The West beyond the West: A History of British Columbia*. Buffalo, N.Y.: University of Toronto Press. 1991. Pp. x, 428. $35.00.

"British Columbia," observes Jean Barman, "presents an enigma. Its identity is elusive . . . British Columbia is no ordinary place, perhaps no single place" (p. 1). This book is Barman's attempt to come to terms with the factors that give an improbable unity to this complex region. Riven into at least ten distinct physical regions by mountainous geography, torn by tensions between Vancouver and the long-isolated hinterland, racked with confrontations between conservative capital and socialist labor in a resource-based economy, how could this male-dominated (at least to 1914), anti-oriental (at least to 1945) society, which attempted to establish a British enclave in the wilderness some 10,000 miles from England, not only produce a peculiar British Columbian identity but also prove flexible enough to maintain it during rapid changes in the second half of the twentieth century?

This is a fine book, and its strengths lie in its emphasis on social, cultural, and intellectual history. Women, for example, are effortlessly integrated into the story, rather than being singled out for separate treatment; one result is that some aspects of women's history—the suffrage movement, or feminism since the 1960s—are brushed over very briefly. Natives appear throughout, a welcome change from their usual disappearance after the fur trade period, so their concerns have a cumulative impact of considerable force. Similarly, orientals are integrated into the history, so that changing attitudes toward them are understood in context. The complexities of British Columbia's labor history and socialist parties are clarified without undue oversimplification. The fur trade, and the fishing, mining, and timber industries are all handled in a carefully balanced manner. This survey is an excellent introduction to the important research that has been undertaken over the last quarter-century, not least of which is the author's own work on education.

Also among the strongest features of the book are its handling of demographic change and urban growth and the ways in which these factors have changed the nature of British Columbian society. Another feature deserving comment is the excellent balanced treatment of the politics of the last forty years, in a province where most commentary on the period tends to be highly partisan. Barman demonstrates clearly the powerful impact on the province of the transportation and communications revolution of that period.

The weaknesses that strike me are two. One is the lack of hard data on the economy, which also is reflected in the otherwise excellent and extremely useful tables at the back of the book. Of twenty tables, only two deal with the economy, one on gross domestic product, the other on mining production. There is nothing on fishing, timber, manufacturing, or ex-

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ports; and curiously, the text does not reflect the fact that coal and petroleum/natural gas have come to be far more valuable in the British Columbian economy than all other forms of mining. Related to this is a second concern: the lack of information on British Columbia in the context of the Pacific Northwest states, transborder commerce, and American investment, on the one hand, and about growing Japanese and other Asian trade and investment on the other. The dependence of British Columbia's resource-based economy on foreign trade is noted, but there is little substantive discussion.

Nonetheless, Barman's work is a pleasure to read and easily supersedes earlier works by Margaret Ormsby and Martin Robin; it is now the introduction of choice to the history of British Columbia.

DAVID J. HALL
University of Alberta

ALLEN MILLS. *Fool for Christ: The Political Thought of J. S. Woodsworth*. Buffalo, N.Y.: University of Toronto Press. 1991. Pp. xiv, 301. Cloth $50.00, paper $18.95.

No Canadian politician, living or dead, has a reputation to match that of James Shaver Woodsworth (1874–1942). He is often described as a "saint" or a "prophet," and not only by members of the Co-operative Commonwealth Federation (CCF), whose first leader Woodsworth was, or its successor, the New Democratic Party (NDP). To many Canadians of all political stripes Woodsworth still appears as a man of unsullied principle and uncompromising socialist and democratic convictions.

Although Allen Mills is an admirer of Woodsworth, he is far from uncritical, and in this book he presents a balanced view. Believing that Kenneth McNaught's fine biography, *A Prophet in Politics* (1959), still has much to offer, Mills focuses on Woodsworth's ideas rather than on the events of his life. Having ransacked Woodsworth's correspondence, speeches, and writings, he depicts a man much more complex in his views than received opinion holds him to be.

The book consists of five long chapters and a conclusion. The first chapter takes Woodsworth into young adulthood and the Methodist ministry. The second traces the changes in his thought as he moved into social work and journalism, and then, in 1921, into the House of Commons. Three chapters deal with Woodsworth's ideas in the 1920s and 1930s, culminating in his decision to oppose Canada's entry into the war in September 1939, a step that effectively ended his leadership of the CCF.

Mills offers a thoughtful and carefully nuanced account of the twists and turns of Woodsworth's mind. He certainly did not suffer from the "foolish consistency" that Ralph Waldo Emerson held to be "the hobgoblin of little minds." Yet there were constants in his thought. He was Anglo-Canadian and suspicious of foreigners. He lost his faith but maintained his Christian identity: "The social gospel was the stimulus to Woodsworth's socialism" (p. 253). He was statist, gradualist, and anticommunist. He thereby cleared a path for others to follow: "His gradualism blends agreeably with the NDP's later penchant for revisionist liberalism and social democracy" (p. 259).

Mills makes a major contribution to our understanding of Woodsworth. Still, some topics get undeserved short shrift. Mills virtually ignores Woodsworth's response to the 1935 "New Deal" of the Conservative Prime Minister, R. B. Bennett. Arch Dale, cartoonist of the *Winnipeg Free Press*, showed Bennett getting into bed with Woodsworth while saying "Move over!!" What did Woodsworth think about this? Mills offers no more than the passing comment that Woodsworth supported the New Deal measures. And Mills fails to mention *Social Planning for Canada* (1935), prepared by the League for Social Reconstruction. Woodsworth, the League's honorary president, wrote in his foreword to the book that "it should be of great service in the formulation of the future policies of the CCF." Mills might have said something about the influence that the book had on Woodsworth's own thought.

Scholars interested in Canadian politics or the Canadian Left will find this book indispensable. General readers are advised to read the biography by McNaught instead.

MICHIEL HORN
York University

PAUL AXELROD. *Making a Middle Class: Student Life in English Canada during the Thirties*. Buffalo, N.Y.: McGill-Queen's University Press. 1990. Pp. 269. $34.95.

The relationship between American higher education and the making of a new middle class is well established in the literature. I have argued for a "culture of professionalism" in the later nineteenth century (*The Culture of Professionalism: The Middle Class and the Development of Higher Education in America* [1976]), and David O. Levine for a "culture of aspiration" between the world wars (*The American College and the Culture of Aspiration: 1915–1940* [1986]). In the context of Canadian universities during the 1930s, Paul Axelrod now develops the subject further with an informed understanding of the complexities of class behavior, cultural values, generational change, institutional restrictions, and socioeconomic insecurities. Only in emphasis does Axelrod's account differ from the American one. The more conservative social structure in Canada restrained students in contrast to their more freewheeling U.S. counterparts, and Canadian industrial developments occurred later than in the United States.

On the surface the scenario is a familiar one. "Middle-class" families whose primary earners were

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in service, white-collar, or modest professional occupations were neither rich nor poor, neither privileged nor outsiders, and were deeply worried about the future of the economy. They turned to higher education to secure for their children the status and respectability of careers that promised both long-term security and mobility. A job market for these graduates (banks, insurance companies) began opening up where it had not previously existed.

Appropriately, the students themselves are the leading performers in Axelrod's account. He holds that a common student experience during these years of the Depression outweighed significant institutional differences. Neither defeated prisoners in a social system nor heroic individualists, the students were fashioning a generational "middle-class" voice of their own, rebellious in terms of aspiration, respectful in terms of the culture of professionalism. Axelrod is at his best describing the ambiguity and incongruity experienced by the students. In the academic culture, for instance, students pursuing a professional course fell short of satisfying the paternalistic college president's mission to cultivate character and launch good citizens. In the professional culture, the rhetorical ideals of progress, objectivity, and altruism bumped up against teaching methods that depreciated critical thinking and a curriculum devoid of public policy issues. The political realities of good-old-boy self-interest and professional jealousy dominated the guilds, barring women, racial minorities, and Jews. Indeed, anti-Semitism and exclusionary practices prevailed as Jews enrolled in professional courses in significant percentages. In the student culture and extracurriculum, the popular styles of the day accentuated appearance over substance, conformity over commitment. These characteristics stood in contrast to the increasing independence and sophistication of students who worked to make expenses, were capable of satirizing the higher education enterprise, and were disillusioned with world leadership as the war approached.

My only reservation about this solid work concerns a matter of historical judgment. Axelrod's repeated invocation of the social categories of mainstream conformity on the one hand and personal independence on the other begins to seem shallow. Only infrequently do the students materialize as subjects with special histories of their own, passing through higher education and making friends as a privileged (in the best sense) rite of passage. The social historian's demographic data is inadequate to convey the meaning of where lower-middle-class students thought they were coming from and what they thought they were up against, especially in hard times. Looking back from the post–1960s generation, Axelrod is disappointed that the 1930s generation students were not more politicized, more intolerant of discrimination and inequality, more cultural pluralists and civil libertarians, in short more radical. He would make the student's life harder and more insecure than it was.

BURTON J. BLEDSTEIN
University of Illinois, Chicago

GRAEME PATTERSON. *History and Communications: Harold Innis, Marshall McLuhan, the Interpretation of History.* Buffalo, N.Y.: University of Toronto Press. 1990. Pp. 251. Cloth $40.00, paper $16.95.

In an engaging afterword, Graeme Patterson admits his initial distaste for the works of Harold Innis and Marshall McLuhan. He changed his attitude after he participated in a group-taught course coordinated by Natalie Zemon Davis on "Man in Society," which led via McLuhan to Innis and to what Patterson describes as "a second liberal education" (p. 220). Eventually, Patterson saw how the approaches of Innis and McLuhan could be applied to the work that he had already completed in his Ph.D. dissertation, "Studies in Elections and Political Opinion in Upper Canada" (University of Toronto, 1969). The afterword is a disarming account of an extended exercise in maturing insights.

Also disarming is Patterson's refusal to succumb to a McLuhanesque style that, he allows, has "done much to exasperate and alienate many intelligent people" (p. 211). (But has he always resisted the temptation? Surely the reference to the "Supercession of one medium . . . by another" [p. 33] is not a typo.) Instead, Patterson endeavors to proceed "as logically, linearly, and sequentially" as possible (p. 211). Above all, that the author feels obliged to provide an afterword is attractive. It acknowledges the paradoxical quality of the material and the provisional nature of the accomplishment. At the same time, however, it betrays an uncertainty of purpose.

In keeping with his frankness, Patterson confronts the question of readership. He notes that, while "the historiographical arguments . . . are . . . primarily addressed to the professional historians of Canada," he would "also like to reach a wider public" (p. 224). One has to wonder about the first aim. Those professional historians open to an Innis/McLuhan perspective will already be familiar with much that is offered here.

Chapters 2 through 4 describe elements of their approaches. Singled out for discursive treatment are the non-linear, non-sequential style that can serve so effectively as a teaching device; the figure/ground contrast; the importance of context; and the cliché/archetype distinction. These insights are then applied in chapter 5 to the Upper Canadian myths of "family compact" and "responsible government." The final chapter covers much the same ground, this time seeking "to explain the rebellion [of 1837] in terms of communications" (p. 184). These chapters, which hint rather than argue, will preach to the converted.

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Perhaps the most substantial argument is in chapter 1, where Patterson convincingly maintains that the early (pre–1950) and the late Innis are not different writers, but that a consistent approach marks the entire corpus. But this chapter had already been published in *Canadian Literature* ("Harold Innis and the Writing of History," [Winter, 1979], 118–30).

And what of the second aim? Clearly, "a wider public" is, well, too wide. The public must be circumscribed, for the book makes severe demands on any reader; in addition to a grasp of Canadian history and historiography, a knowledge of Canadian geography would be helpful, for the importance not only of Yonge and Dundas streets is underlined, but also that of Kettle Creek.

Once the readership has been defined as senior undergraduate and graduate students in Canadian history, it can be said that this is a valuable book. Because Patterson constantly sets Innis and McLuhan against other writers, notably Donald Creighton and Northrop Frye, the strengths and weaknesses of different approaches emerge. Open-minded and imaginative students will benefit from this study, for its engaging quality is not limited to its afterword.

JOHN L. FINLAY
University of Manitoba

LATIN AMERICA

D. A. BRADING. *The First America: The Spanish Monarchy, Creole Patriots, and the Liberal State 1492–1867*. New York: Cambridge University Press. 1991. Pp. xviii, 761. $44.50.

This is a book full of conundrums, the first of them in the title. For if D. A. Brading intends by "The First America" to set colonial Spanish America off from Anglo-America, he strongly suggests in his text that, at least in the analytical categories he uses, Spanish America divides into two: Mexico and South America.

Creole patriotism—that sense in American-born Spaniards of forming social and ethnic groups that belonged to some American place—is declared as the "primary theme" of the work (p. 2). But Creole patriotism, Brading persuasively argues, was a far more flourishing plant in Mexico than in South America, so that when the sentiment for independence arose in the early nineteenth century Mexicans were able to convert creole patriotism into a political ideology. South Americans, lacking from colony to colony any strong sense of nationhood, tended to draw for guidance on liberalism—an imported creed whose multifaceted quality made it but a whirling political compass.

Creole patriotism may be the primary concern, but the book approaches that subject along at least three roads, each of which at times seems to be the highway: the colonial historiography of Spanish America, political ideology, and actual political events in the American empire.

The method Brading uses is a chronological study of many who left, by writing, action, or a blend of the two, a stamp on Spanish America's political past (and, in some cases, its present). Brading has put phenomenal effort into rereading the chroniclers, both the standard figures and many lesser, rarely remembered, men. He writes of them all afresh, with minimal reference to the critical literature. He also examines, often with great brilliance, the deeds of some who, although they wrote, were first political or administrative actors: Toledo, Solórzano y Pereira, Gálvez, Bolívar, Juárez, and others. The chapter on Juan de Palafox y Mendoza is a veritable *tour de force*. Rarely has the hothouse, baroque world of mid-seventeenth-century Mexico been more vividly conveyed. The baroqueness of other aspects of creole life glows through the pages given over to saints and intellectuals.

Creole patriotism had its origins, according to Brading (and others), about 1600, in a feeling of dispossession among descendants of conquerors. The wealth, lands, and Indians that their forefathers had won were taken away by governmental decree and by competition from later immigrants from Spain. Doubtless, too, creoles' malaise was a reaction to the rise in the sixteenth century of what Brading calls the "Imperial tradition"—a trend in historiography and in state ideology that drew first on the triumphs of Spanish conquest and later on convictions of the divinely granted, indeed providentially given, power of the Spanish monarchy. A natural tension existed between these two ideologies, which were far from clearly or always separate. Brading refers to the "perennial ambiguity of creole patriotism" (p. 459). Creoles in Mexico came, on occasion, to glory in the splendor they perceived in Aztec state and culture, finding there a basis for a general Mexican nationhood. But how was past native magnificence to be reconciled with present native abjection?

This central ambiguity of Brading's principal subject perhaps accounts in part for the impression of lack of focus, or at least shifting focus, that the book leaves. The prologue summarizes the main points efficiently, but there is no conclusion to bring together the vast erudition and the quantity of information provided. The index, consisting largely of name entries, is of little help for synthesis. One almost has the impression that Brading offers his twenty-nine chapters as primary material rather than as parts of a continuous argument. Certainly many will consult the book in that way, looking for sharp summaries of, say, Bartolomé de Las Casas's life and thought, or of the radical liberalism of the Mexican Reform. They will be well satisfied. But such splendid learning and intelligence as are shown here deserve better.

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MARIE LAURE RIEU-MILLAN. *Los diputados americanos en las Cortes de Cádiz (Igualdad o independencia).* (Biblioteca de historia de América, number 3.) Madrid: Consejo Superior de Investigaciones Cientificas. 1990. Pp. xxiii, 438.

Marie Laure Rieu-Millan has produced not a work of collective biography of the American representatives in the first Spanish constitutional experiment (1810–14) but instead a systematic analysis of their ideas and objectives and positions taken on a wide range of issues that affected the "España de Ultramar." Based on a comprehensive review of printed primary and secondary sources as well as archival holdings in Spain, Ecuador, and Peru, the book is as close to a definitive treatment as we are likely to see. The subject itself, of course, is far from being historiographically virgin territory. Scholars such as Timothy Anna and Michael Costeloe have dealt with the role of the Cortes and of the Constitution of 1812 as part of the larger story of Spain's efforts to preserve its American empire, and María Teresa Berruezo in *La participación americana en las Cortes de Cádiz (1810–1814)* (1986) reviewed the activity of the same group of deputies, though with more of a biographical emphasis than in the present volume. One is tempted to wonder whether the impressive research effort that went into this study might not have been better devoted to something else. What is beyond doubt is that there will be no need in the foreseeable future for anyone to go over all this again.

The picture that emerges from Rieu-Millan's research is one of an able group of men, conscientiously devoted to serving their constituents and genuinely hopeful that constitutional liberalism would provide both a means to remedy the grievances of Spanish Americans and a basis for retaining a linkage between America and Spain. Some were ideologically more liberal than others; as a group, the Americans were a bit less liberal than peninsular deputies. The deputies also varied in the firmness of their commitment to imperial unity, which all but a few insisted should be defended primarily by peaceful means. One fact that comes out clearly is that often their strongest commitment of all was to their *patrias chicas*, usually at the expense of the interest of neighboring provinces. In general, they sought a greater devolution of power to the local level, and although they did not formulate this demand in federalist terms, they in some cases expressed an embryonic federalism. Equally clear, and hardly surprising, is the instinctive identification of American interests with those of the creole upper class to which virtually all the deputies belonged. They strongly opposed the discrimination against *pardos* in the constitutional definition of citizenship, but not so much out of belief in racial equality as in the expectation that any enlargement of the representable population base would mean the election of more creoles. They were outvoted on that issue by the peninsular deputies, intent on limiting the size of American representation, and they lost some other arguments too; but the author concludes, convincingly, that the constitutional interlude brought positive advances for America as well as European Spain and was so perceived by the deputies from overseas.

Few of the findings in this study are strictly new, and the general interpretation is mostly familiar. Many works of greater importance for Latin American history are never reviewed in the *AHR*. The fact remains that this is a very good book, well researched and admirably executed.

DAVID BUSHNELL
University of Florida, Gainesville

RAMON A. GUTIÉRREZ. *When Jesus Came, the Corn Mothers Went Away: Marriage, Sexuality, and Power in New Mexico, 1500–1846.* Stanford: Stanford University Press. 1991. Pp. xxxi, 424. Cloth $49.50, paper $16.95.

This stimulating work by Ramon A. Gutiérrez about Spanish marriage practices in New Mexico and the critique of Franciscan evangelism on the frontier illustrates some important points about the current state of social historical writing. We can still recognize the original commitment of social history to represent the perspective of the masses who do not write in Gutiérrez's stated desire to "give vision to the blind, and give voice to the mute and silent" (p. xvii). What is different is the scholarly context in which his study is framed. This work is not written mainly to place itself comparatively in the now rich historiography of Spanish America on the very same questions. Indeed relevant works such as Daisy Rípodas Ardanz's major treatise on the adaptation of Spanish marriage practices to the New World (*El matrimonio en Indias* [1977]) are not cited. Rather, this volume orients itself in terms of a vibrant area of contemporary interdisciplinary concern with an attempt to revive the histories of colonialism with a particular sensitivity to how colonial subjects are constructed both by themselves and by colonizing powers. Furthermore, historians combine their styles of theory and research to probe the subtle ways that subaltern peoples resist forms of colonial domination through struggles in which variant kinds of gender, racial, and cultural identities are at stake.

This work originated as part of a generational cohort of historians-in-the-making researching during the late 1970s in the national archives of Mexico. By now several studies have been produced from within this cohort. Archdiocesan marriage records on which Gutiérrez's study depends also have been the basis of several of these other works.

Gutiérrez makes use of these records to address matters such as marriage patterns among the Spanish families of Santa Fe, the negotiation between honor as status and as virtue in the eighteenth century, the

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role of the diocesan clergy, and the status of broken marriage promises. Gutiérrez's arguments are poignantly illustrated with tales of families and young people as they negotiate family interests, their own desires, social customs, and the formal requirements of the Catholic church. Yet by not sufficiently developing the comparative context of his research with the work of others, his portrait of New Mexico's Hispanic society tends toward stereotype, missing the nuanced differences with other local situations in the same period.

As to its framing orientation within new attempts to revise the histories of colonialism, this work exemplifies the themes of this literature—the violence done to New Mexico's several indigenous societies through Hispanic conquest and Franciscan missionizing and the complex resistances of these societies to such domination. Here Gutiérrez provides a critical history of changes in the sexual division of labor, the erosion of the authority of hunt chiefs, and the reorganization of ritual and religious life culminating in the Pueblo revolt of 1680. In this respect Gutiérrez's work represents the mainstream in this newer history of colonialism. It is increasingly clear, however, that the strong focus on oppression, resistance, and cultural survival is not enough. Indigenous voices are predominantly represented by official records and hence are not present in an unmediated way in the sources. This means that points of view developed in a study such as this must be more flexible, varied, and mobile. As Donna Haraway has recently noted, "to see from below is neither easily learned nor unproblematic . . . The standpoints of the subjugated are not 'innocent' positions . . . They seem to promise more adequate, sustained, objective, transforming accounts of the world. But *how* to see from below is a problem requiring . . . much skill" (*Simians, Cyborgs, and Women* [1991], 191).

What this means for scholarship on Spanish America, and, for that matter, any other area of early colonization, is increased attention to institutions of domination along with the everyday lives of the dominated. Far from being a return to elite history, this leads to such recent work as Vicente Rafael's history of Spanish colonization of the Philippines (*Contracting Colonialism* [1988]) and Jean Comaroff and John Comaroff's study of the Tswana people of southern Africa (*Of Revelation and Revolution* [1991]), both based on intensive work with missionary records and emphasizing a completely shared system of colonial engagement. Although it is a powerful view of resistance and the continuity of indigenous societies in colonial New Mexico, Gutiérrez's study fails to achieve a vision of the emergence of a hybrid society in which various accommodations and resistances among both conquerors and subjugated were formative. The equally complex story of accommodation and the forging of a hybrid order is thus obscured in the strongly drawn story of subaltern cultural resistance and survival. The archival records themselves, created by colonial power and by which the historians' knowledge of this resistance emerges, require a more complex canvas of multiple perspectives for studies like this.

PATRICIA SEED
Rice University

PATRICK J. CARROLL. *Blacks in Colonial Veracruz: Race, Ethnicity, and Regional Development.* Austin: University of Texas Press. 1991. Pp. xv, 240. $35.00.

As a result of post–1910 Mexico's emphasis on integrating Indians and building the myth of a mestizo nation, the experience of Afro-Mexicans has remained a largely ignored topic of research. Patrick J. Carroll's book attempts to fill this gap by focusing on African slaves and their descendants in the region of Veracruz between 1519 and 1830. The study revolves around three related themes: the evolution of the slave and free population of African origin in Veracruz; the role of Afro-Veracruzanos in the development of the region; and the material and ideological influences that shaped Afro-Veracruzanos' lives and Veracruz society.

The first half of the book describes demographic and economic changes in Veracruz from the time of Indian and European contact until after independence. The dramatic loss of Indian population simultaneous to the introduction of sugar plantations in the early 1500s prompted the Spanish elite to turn increasingly to African slave labor. The slave trade reached a peak between 1597 and 1610 with the purchase of about 400 slaves, mostly from Angola. By the mid-seventeenth century the lack of excess capital and the renewed growth of the indigenous population led to the decline of slavery and the rise of voluntary wage labor.

The second half of the book examines the ways slave and free Afro-Veracruzanos influenced their socioeconomic and political environment. Carroll compares two divergent routes to freedom in the region. The first, characterized by violence, is linked to the district of Córdoba, where slavery developed only in the late seventeenth century. Between 1725 and 1768, slaves there organized massive rebellions that undermined the slave system, and after 1810 they joined Miguel Hidalgo's and José María Morelos's populist revolts that promised abolition. The slaves also founded maroon communities that survived well into the nineteenth century. A less violent and more evolutionary process occurred in the district of Jalapa (and most of Mexico). Economic change, mostly the establishment of trade fairs, brought immigration and new labor opportunities to Afro-Veracruzanos, which made slavery obsolete. After independence a few Afro-Veracruzanos acquired wealth and political power. In 1830, however, white Mexicans regained full control of the region and kept

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castas (people of mixed origin) and Indians in subordinate positions until the 1910 revolution.

Carroll has collected a wealth of demographic and economic data in the Archivo General de la Nación in Mexico and Veracruz parochial archives that are summarized in impressive appendices. It is regrettable, however, that this revised version of his Ph.D. dissertation could not fully incorporate approaches suggested by recent studies of the Afro-Latin American experience. Carroll's discussion of ethnicity provides little insight into Afro-Veracruzanos' actual sociocultural life. Apparently, his position is that the retention by Afro-Veracruzanos of African ethnic traits, rather than helping them cope with slavery, was negative and prevented them from integrating into the broader society. Similarly his analysis of race is limited to an approach that relies on the categories established by priests and notaries without attempting to investigate how Afro-Veracruzanos perceived themselves.

These points should not obscure the fact that this well-documented book is necessary reading for those interested in race and the African diaspora in colonial Mexico.

ALINE HELG
University of Texas, Austin

SHIRLENE SOTO. *Emergence of the Modern Mexican Woman: Her Participation in Revolution and Struggle for Equality, 1910–1940.* (Women and Modern Revolution.) Denver, Colo.: Arden. 1990. Pp. xvi, 199. Cloth $26.50, paper $16.95.

ELIZABETH SALAS. *Soldaderas in the Mexican Military: Myth and History.* Austin: University of Texas Press. 1990. Pp. xiii, 163. Cloth $25.00. paper $11.95.

In two decades of prolific, pathbreaking research on the Mexican Revolution, little work has been done on women. Scholars like peasants. They view them as men rather than members of families, which were the organizing units of rural life. Feminists' allegations that the revolution was no revolution for women seem to have sanctioned scholarly neglect. The voices of middle-class women who met in Yucatán in 1916 to discuss how to advance women in a modernizing society seem to have been drowned out in the rifle fire and wheeling and dealing of sauntering, big-bellied leaders who successfully mobilized men around an issue that cut deep into the sensibilities of Mexican women. Anticlericalism became an attack on the Catholic church, religion, and family rights.

Although neither of the books under review is contextualized in recent Mexican historiography, both are steps toward a history of women and gender. Shirlene Soto describes the women who struggled to increase employment and educational opportunities for women, to secure legal and political equality, and to address issues of family and children between 1900 and 1940. Middle class and products of Porfirian schools, these women were so isolated that they had to rely on "progressive" male leaders such as Salvador Alvarado to give them voice, and on new government bureaucracies in education and health to express their creative energies. At Alvarado's feminist congress of 1916, women were divided on the question of suffrage. Many believed that women's foray into politics would threaten the sanctity of the home. The feminist movement may have gained more adherents had the government not attacked the church. Middle-class women who were progressive on women's political rights tended to side with the government while the majority organized against the revolution in defense of faith and family. Soto recognizes this problem. It divided Mexican women and delayed women's voting in national elections until 1958 because male leadership feared their conservative ballots. Soto could have explored the problem in greater depth. Why did Alvaro Obregón and Plutarco Elías Calles, descending from the capitalist north to modernize Mexico, show such powerful disdain for women in their antireligious excesses when a truly modern discourse would have suggested greater consideration. Social forces prevailed. Thanks in large part to the energies of Mexican women, the church emerged intact. The church surely gained more from the Mexican Revolution than did women.

Soto could also have explored the impact of a massive rural rebellion on the agenda for women's rights. The patriarchal mores of the countryside exploded in an orgy of violence against women between 1910 and 1917. Contrary to Soto's assertion that the violence forced women to organize for their rights, it weakened their capacities to do so. The participation of women in the fighting is the subject of Elizabeth Salas's study of the *soldadera*. Women participated in two ways. First, they moved the campesino household onto the battlefield and into the camps—foraging for and preparing food, laundering, gathering firewood, setting up altars, praying, carrying the dead and wounded from the battlefield, nursing, loving, and burying them beneath crosses of cactus thorns, giving birth, and dying. These women spied, smuggled guns and ammunition, and shouldered rifles in battle. The second group of women chopped off their hair, adorned themselves with braids of bullets, lowered their voices, and mounted horses to make war. It became a singular task for male leadership to get all women out of the armies: to banish the female "campfollowers," who were likely to interfere with "modern" military maneuvers; to remove the women officers and to transform them in battle lore and corridos into what they clearly were not: gentle and accessible objects of male desire. As the services rendered by *soldaderas* were professionalized in the all-male army forged by the state after 1920, the women who had served went uncompensated while male veterans received pensions and health care.

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Salas suggests the construction of a developmentalist discourse around gender that distorted the real participation of the *soldaderas* in the revolution and rendered them creatures of nature, not rationality. Although the *soldadera* had a long history as a worker paid a wage by the soldier to secure him food, in the late nineteenth century the psychologist Julio Guerrero depicted the *soldadera* as primitive, backward, and the object of base sexual desire. Her paid work was delegitimized. Women belonged at home. The revolutionary generals expelled them as impediments to modern warfare, and, afterward, the women who had served could obtain no monetary compensation because they were seen as sex objects attached by nature to men as wives, family members, or prostitutes. In art and popular culture after the revolution, the *soldadera* was either portrayed as a shameless prostitute, a submissive, faceless wife-mother, or an Adelita, sweetheart of the *tropa*. The fearless female soldier, Juana Gallo on the movie screen, was tamed by male sexual prowess and turned into a self-abnegating patriot, a docile wife and mother. Salas contrasts this male construction with the writings and opinions of female artists and ex-*soldaderas*. She reveals a more complex picture of dislocation, opportunity, suffering and abuse, ingenuity, and, sometimes, remarkable, enduring empowerment.

These books have their shortcomings. Soto covers territory already mapped by Ana Macias (*Against All Odds: The Feminist Movement in Mexico to 1940* [1982]). Salas decontextualizes women's participation in war and rebellion by reifying it and making dubious linkages between pre-Hispanic war goddesses/earth mothers and the Virgin of Guadalupe/Maria Insurgente. Nonetheless, both books are provocative studies that help us to construct a gendered understanding of the Mexican Revolution.

MARY KAY VAUGHAN
University of Illinois, Chicago

OBIKA GRAY. *Radicalism and Social Change in Jamaica, 1960–1972*. Knoxville: University of Tennessee Press. 1991. Pp. xiv, 289. Cloth $42.50, paper $19.95.

The value of this book lies in its compelling historical account of the radical Left as a political actor during the crucial first decade of Jamaican independence. Besides Trevor Munroe's *Jamaican Politics: A Marxist Perspective in Transition* (1990), Obika Gray has written one of but a handful of books on this subject. Gray's basic argument is that an array of diverse groups (including the militant urban poor, Rastafarians, radical intellectuals, students, Black Power advocates, the newspaper *Abeng*, Marxists, and independent labor union activists) became an urban protest movement manifesting the struggle of the black poor against cultural, political, and economic marginalization by the Jamaica Labour Party (JLP) government. In response, the JLP government became increasingly authoritarian and sought to neutralize and intimidate "Communists" and others guilty of "subversion." Despite its limited political space, the radical Left had significant impact on societal consciousness. Its challenge not only broadened political discourse and legitimized political dissent but also led to a significant shift in the balance of class support away from the incumbent JLP, in favor of the opposition Peoples National Party (PNP). According to Gray, the Left was unable to mount an effective electoral challenge to the dominant JLP and PNP due to ideological polarization within the movement, poor organization, and state repression.

Gray's extended discussion of the post–1972 period is especially valuable, particularly where he describes the "top-down" process of transition in which the newly elected PNP incorporated the ideas of the progressive movement of the 1960s and moved rapidly toward more substantial democratization at cultural, sociopolitical, and economic levels. Gray's interpretation makes it clear that the radical politics of the 1960s was not an aberration but rather a critical link in the evolution of Jamaica's democratic political culture.

Gray makes incisive comments on what he calls "Jamaican exceptionalism," an ideology of the middle-class political leaders and their allies designed to decrease the salience of race in Jamaican politics. As Gray explains, "exceptionalism discredits black nationalism as the subordinate classes are hailed as exemplary racial neuters in a world torn by ethnic disorder and strife" (p. 82). With refreshing candor, he explodes the myth of racial harmony emphasizing that race was a vexing problem in the 1960s, as it is today. Coercive state power, clientelist control, and political patronage all serve to stem challenges to racial and class inequalities.

This book is clearly a contribution to political histories of Jamaica, but at the outset the author demonstrates that it has a distinct theoretical thrust. Gray seeks to contribute to the existing literature on the nature of the Jamaican postcolonial state. In accordance with Carl Stone (*Democracy and Clientelism in Jamaica* [1980]), Gray characterizes Jamaica as an "authoritarian democracy," an anomaly in the Third World possessing features of both pluralism and authoritarianism. Clientelism was central to Stone's formulation, which Gray rejects because he rejects explanations based on clientelism. In his view, such theories overstate the role of personal relationships in the process of power accumulation. For Gray, clientelism is as important as coercion and state power. Gray makes no reference to a significant body of recent scholarship on clientelism in Jamaica that goes beyond Stone, linking clientelism to internal middle-class politics, statism, and external dependency. Concentrating on the personal aspects of clientelism deflects attention from its political economy base.

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Gray does not break new theoretical ground. The additional variables he identifies in his formulation are already incorporated into the recent literature on clientelism and Jamaican democracy. Nevertheless, this book is compulsory reading for those interested in a more balanced perspective on post-independence Jamaican politics. It represents the most lucid, cogent, and detailed contribution on left-wing radicalism and social change in Jamaica during the 1960s that has yet appeared.

CARLENE J. EDIE
University of Massachusetts, Amherst

NICOLAS SHUMWAY. *The Invention of Argentina.* Berkeley and Los Angeles: University of California Press. 1991. Pp. xiv, 325. $34.95.

The breakdown of the Spanish empire in Latin America resulting from the process of independence (1808–26) brought no panacea to the fledgling Latin American republics. They faced enormous challenges, including the collapse of the commercial system, a ravaged economy, continuous foreign and civil wars, and the inability to build legitimate strong states. In this book, Nicolas Shumway discusses how one country, Argentina, built a nation out of the ashes of the former viceroyalty of La Plata, the newest in the Spanish empire. His study seeks not only to show how political leaders and intellectuals defined nationhood in the nineteenth century but also to explain Argentina's precipitous decline as a leading Latin American nation during the twentieth century. His motivation, as eloquently expressed in the preface and epilogue, is to understand the background for Argentina's failure to realize a sense of unifying nationhood to this date.

Shumway takes us through ten chapters covering the period from 1810 through 1880, with frequent references to the periods before and after. During this epoch, two major "guiding fictions" (by which he means the ideas that underpinned models of nationhood) emerged. One was that of triumphant liberalism, which from the start was characterized by an emphasis on the incorporation of Argentina into the international economy, with all its attendant transformations in the social and political fabric of the nation: the adoption of European-inspired models and ideas, an immigration policy that favored, but did not attract, northern Europeans, and an undisguised attempt to do away with local *gauchos* and Indians. The major liberal paradigm was that of "civilization versus barbarism" developed by intellectual and future president Domingo Faustino Sarmiento (1811–88), whereby Argentina was urged, if not forced, to subordinate the countryside and its people to the economic and political domination of Buenos Aires, the major port and later capital city. Shumway traces the development of this dichotomy and shows how it has informed the ideas of liberals ever since. According to this paradigm, the country's Spanish-Catholic legacy, its racially mixed population, and its numerous regional bosses, or *caudillos*, are to blame for Argentina's ills. To justify the repression of these *caudillos*, the banner of civilization is waved, a convenient reason to suppress dissent from the last century to the present.

The other major "guiding fiction" is nationalism. Shumway concedes that this is only a label to classify a motley array of populist, reactionary, nativist, and federalist tendencies, but emphasizes how Argentine nationalism overall is characterized by its distrust of liberalism, its economic protectionism, its anti-Europeanism, and its unabashed preference for strong leaders and authoritarian models. Nationalists prove to be as efficient as their liberal counterparts in justifying their coups and persecution of dissidents. They share the same distrust for the masses but speak in their name. From Juan Manuel de Rosas to Juan Domingo Perón, these nationalists have developed an ideology that seeks to shun foreign influences and, especially in this century, to restore Argentina's once prominent place in the world.

Shumway argues that both "fictions" are ideologies of exclusion that explain the sad lack of consensus that characterizes Argentina to this day. His emphasis is on the continuity of Argentine history since independence, and he succeeds remarkably well in establishing precedents and points of contact between intellectuals and politicians over an almost two-century-old history. Historians may argue that specific economic shocks ranging from nineteenth-century indebtedness to the impact of the Great Depression and through the current stabilization policies can better explain the acrimonious nature of political confrontation in that country. But all are likely to benefit from Shumway's ability to organize a vast array of authors and intellectual currents in a concise book and to provide readers with a nuanced, insightful, and balanced account of Argentina's tragic history.

IVAN JAKSIC
University of Wisconsin, Milwaukee

JOHN KING. *Magical Reels: A History of Cinema in Latin America.* (Critical Studies in Latin American Culture.) New York: Verso, in association with the Latin America Bureau. 1990. Pp. x, 266. $49.95.

John King's introduction to this book aptly describes the major problems of existing English-language works on Latin American cinema. First of all, few scholars have attempted generalizations about the history of cinema for the entire continent. Usually they focus on national cinemas—such as those of Cuba, Argentina, Mexico, and Brazil—that have received either attention at international festivals or

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healthy profits in the Latin American film market. Second, there has been a tendency to concentrate on the Latin American cinema from the 1960s to the present. Revolutionary film movements, such as *cinema novo* in Brazil, the post-Castro cinema of Cuba, or the films of Allende's Chile, have dominated the scholarship, while those productions outside the context of revolutionary situations have been ignored. Thus, national productions in other countries (or in other periods) tend to be absent from the literature.

By the same token, while North American film scholarship has delved into the early decades of the cinema, the same cannot be said for Latin America. The silent era remains blissfully ignored by most English-language writers. Part of the problem, King asserts, is the fact that Latin American film archives have not kept pace with their North American counterparts. As a result, the historical record of the silent era continues to vanish in Latin America. Yet it could be argued that North American film history has faced a similar problem, but that has not stopped North American film historians from the continued search for the full dimensions of early film history.

While King has correctly noted the major problems in English-language American film scholarship, he has done little to correct them. His book dismisses the silent era in the first chapter, and the period from 1930 to 1950 is summarized in the second. Then, contrary to his own advice, King focuses on the oft-chronicled cinemas of Brazil, Cuba, Mexico, and Argentina.

King should be commended for his inclusion of Andean and Caribbean cinema, which are usually lacking from general surveys. Nevertheless, he has failed to solve the problems he has sketched in his own introduction. It is not that the material is not there, since there are several excellent Spanish and Portuguese language studies of the pre–1960s cinema. Similarly, a study of major periodicals and newspapers of the period might provide some of the insights that King hopes to discover. Often, King chooses a novelist for support of his ideas. This is not surprising since Latin American novelists from Gabriel García Márquez to Carlos Fuentes are fascinated with the cinema. But the factual context for many such assertions remains to be seen. Nonetheless, King's work is valuable as a survey of major Latin American cinema movements from the 1960s to the present.

Allen L. Woll
Rutgers University, Camden

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed.

DAVID C. LINDBERG and ROBERT S. WESTMAN, editors. *Reappraisals of the Scientific Revolution*. New York: Cambridge University Press. Pp. xxvii, 551. Cloth $59.50, paper $22.95.

ROBERT S. WESTMAN and DAVID C. LINDBERG, Introduction. DAVID C. LINDBERG, Conceptions of the Scientific Revolution from Bacon to Butterfield: A Preliminary Sketch. ERNAN MCMULLIN, Conceptions of Science in the Scientific Revolution. GARY HATFIELD, Metaphysics and the New Science. ROBERT S. WESTMAN, Proof, Poetics, and Patronage: Copernicus's Preface to *De revolutionibus*. JOHN GASCOIGNE, A Reappraisal of the Role of the Universities in the Scientific Revolution. BRIAN P. COPENHAVER, Natural Magic, Hermetism, and Occultism in Early Modern Science. WILLIAM B. ASHWORTH, JR., Natural History and the Emblematic World View. WILLIAM EAMON, From the Secrets of Nature to Public Knowledge. JAN V. GOLINSKI, Chemistry in the Scientific Revolution: Problems of Language and Communication. HAROLD J. COOK, The New Philosophy and Medicine in Seventeenth-Century England. MICHAEL HUNTER, Science and Heterodoxy: An Early Modern Problem Reconsidered. MICHAEL S. MAHONEY, Infinitesimals and Transcendent Relations: The Mathematics of Motion in the Late Seventeenth Century. ALAN GABBEY, The Case of Mechanics: One Revolution or Many?

ELIZABETH GARBER, editor. *Beyond History of Science: Essays in Honor of Robert E. Schofield*. Bethlehem, Pa.: Lehigh University Press or Associated University Presses, Cranbury, N.J. 1990. Pp. 325. $48.50.

ELIZABETH GARBER, Introduction. MELVIN KRANZBERG, To Robert E. Schofield: A Personal Tribute. RUSSELL MCCORMMACH, Henry Cavendish on the Proper Method of Rectifying Abuses. MARTIN J. KLEIN, Duhem on Gibbs. MICHAEL M. SOKAL, Life Span Developmental Psychology and the History of Science. CLARK A. ELLIOTT, Collective Lives of American Scientists: An Introductory Essay and a Bibliography. VIRGINIA P. DAWSON, The Limits of Observation and the Hypotheses of Georges Louis Buffon and Charles Bonnet. PATSY GERSTNER, The Influence of Samuel George Morton on American Geology. ALAN J. ROCKE, Methodology and Its Rhetoric in Nineteenth-Century Chemistry: Induction versus Hypothesis. ELIZABETH GARBER, Siméon-Denis Poisson: Mathematics versus Physics in Early Nineteenth-Century France. EDWIN T. LAYTON, JR., Newton Confronts the American Millwrights, or, Action and Reaction Are Not Always Equal. DAVID F. CHANNELL, W. J. M. Rankine and the Scottish Roots of Engineering Science. ANDREW J. BUTRICA, The "Rediscovery" of Kirchhoff's Circuitry Laws. EDWARD JAY PERSHEY, Warner and Swasey at the Naval Observatory: A View of the Science-Technology Relationship. DARWIN H. STAPLETON, The Rise of Industrial Research in Cleveland, 1870–1930. ORVILLE BUTLER, From Local to National: Time Standardization as a Reflection of American Culture. ALAN I. MARCUS, From Ehrlich to Waksman: Chemotherapy and the Seamed Web of the Past. HARRY J. EISENMAN, One Culture: Creativity in Art, Science, and Technology. DAVID TOPPER, Natural Science and Visual Art: Reflections on the Interface.

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ARTICLES

To the Editor:

Like many readers who are Middle East specialists, I was pleased to see an entire issue of the *AHR* (December 1991) devoted to the modern Middle East, defined primarily as the nineteenth and twentieth centuries.

Articles, even book reviews, in the Middle Eastern field have been relatively rare in past issues of what is the flagship journal of the historical profession in the United States. What explains that under-representation? Interested readers might find an answer in the editor's remarks introducing the December issue (p. iv). The genesis of that issue, he said, "was the recent war in the Persian Gulf." He added that he hoped that "the essays [would] direct . . . readers to scholarly works that would allow them to deepen their knowledge of Middle Eastern history." Despite the laudable goal enunciated in the latter remark, the former remark gives away an attitude about the Middle East common to many historians who have no specialized knowledge of the area: that it is important only if there is a war going on there. Specialists, however, also follow this line of thought. In a communication published in the April issue of the *Review* [p. 680], one of Carter Findley's arguments for Turkey's importance is that that country "has historically emphasized its ties to the West much more than any other Islamic country has. Turkey played a major role in the Gulf crisis, as a member of NATO." To be sure, this is not his only argument, but it is an unfortunate one, and unnecessary.

The fact is that Middle Eastern history is still too often regarded as exotic, the people involved as strange, the cultural and religious traditions as monolithic. The reasons for this state of affairs are complex: we Middle East specialists are certainly at fault for being what our best-known scholar, Albert Hourani, once called "priests of a mystery"; on the other hand, non-specialists should now realize that in the new world order, however defined, this exoticism is potentially harmful. As professionals, all of us must concede that our subjects of study are "normal," that their pasts are explainable and comparable, and that, like a quarrelsome family, the historical profession will continue to examine those pasts with different questions and methods. I for one am pleased to learn that the Middle East will be an integral part of the forthcoming third edition of the *AHA Guide to Historical Literature*. Following this route will be better for us all, historians and lay persons, rather than continuing in the old habit of seeing other areas and other traditions as important only insofar as they have an immediate impact on ourselves. Such solipsism, which is normal among human beings, is after all one of the things that the study of history attempts to overcome. If we fail in that task, should we not now expect a sudden flowering of studies of southeast Europe? Or of Somalia? Or of Los Angeles?

Karl K. Barbir
Siena College

To the Editor:

As a candidate for the U.S. Senate from California this year on a third-party ticket, I read William Leuchtenburg's article with keen interest [*AHR*, 97 (February 1992): 1–18].

Even though I was born at the time that Leuchtenburg was beginning his career, I also have an extensive record in the public realm, which I will abbreviate in the interest of space: Special Counsel to Local 1199 of the Hospital Workers' Union in New York City (one of the few U.S. unions with left-wingers in leadership) with a portfolio in political action; Executive Director of the National Conference of Black Lawyers, where I was asked to mediate the civil war in Sudan and worked closely with both SWAPO of Namibia and the African National Congress of South Africa; as a human rights lawyer, I participated in 1987 in a trailblazing inquiry on vigilantes in the

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Philippines that was highly critical of U.S. foreign policy; as a journalist, I covered the elections in Namibia in 1989 for a newspaper in Helsinki and wrote a regular column for a number of African-American newspapers on political questions; currently, I comment regularly on political and diplomatic questions for Pacifica radio and write periodically for the *Los Angeles Times*. My opinion—drawn from bitter experience and direct observation—is that this type of more overtly "left" activity in the public realm is not deemed as acceptable as Leuchtenburg's political work—and this is a problem within our profession and this nation.

Despite our common fondness for the public realm, I have some questions about Leuchtenburg's analysis. He objected to the Organization of American Historians opposing the Contra war in Nicaragua and the Gulf War. Fair enough. I wonder, however, if he would have objected to historical associations in Germany opposing the Holocaust or wars abroad or such associations in South Africa opposing apartheid or wars in Mozambique and Angola or such associations in the then-USSR opposing war in Afghanistan. I do not think the question is as easy to answer as he apparently assumes.

He also registers opposition to "any effort to politicize this organization [the AHA] or to impose favored orthodoxies on the classroom." And he recalls that "it was not so long ago that most members of this association sanctioned the institutions of white supremacy that emerged out of Reconstruction and an even shorter time ago that not a few historians were apologists for Stalin's despotic regime." The salient difference is that the "white supremacists" and their "favored orthodoxies" were represented liberally on most U.S. history faculties, while the "apologists" or any who veered from the "favored orthodoxies" of Cold War liberalism, conservatism, and their variants were "blacklisted" and pilloried. It should be noted that the Americans for Democratic Action, for which Leuchtenburg toiled, was an avatar of Cold War liberalism.

One of the questions that historical associations should be debating is not only the propriety of taking positions on foreign policy matters but how to open up further the profession to ensure that not just "favored orthodoxies" are represented in the academy. As a graduate student at Columbia more than a decade ago, I can recall eminent professors wondering about the hiring of Eric Foner, not because of the quality of his scholarship and teaching but more the presumed nature of his political pedigree. A person of my ethnicity and outlook—squalls about the "tyranny of the politically correct" notwithstanding—is as rare on faculties as snow in Los Angeles. The academy remains "no ivory tower."

And this is not just an academic issue. The purge of the professoriate tilted academic discourse to the right and placed undue focus on the Soviet Union, which was an important part of the process that has led to distorted government spending and the decline of the United States (particularly, declining spending for higher education—an important issue, I would think, for a historical association), the placing of Japan and Germany in the passing lane, and an increasingly unsettled planet.

I agree that historians should not "eschew subjects of contemporary concern or avoid the political arena." The point should be to broaden our definition of what is acceptable behavior in these realms—not to mention inclusion within scholarship of something other than "favored orthodoxies."

GERALD HORNE
University of California, Santa Barbara

WILLIAM LEUCHTENBURG REPLIES:

Yes, of course, the circumstance of the historian would be different in a totalitarian society, not only in the vile tyrannies Gerald Horne mentions but in those he does not—of Stalin, of Mao, of Idi Amin. But the United States is not, and never has been, a totalitarian country. Moreover, no matter what the circumstances, the historian never has license to distort the truth in the interests of a political cause nor to impose his agenda on his colleagues.

WILLIAM E. LEUCHTENBURG
University of North Carolina, Chapel Hill

TO THE EDITOR:

Patricia Nelson Limerick's thoughtful and well-written discussion of the theme of multiculturalism as reflected in several recent books on Hawaiian history was illuminating and provocative [*AHR*, 97 (February 1992): 121–35]. In addition to explicating and analyzing the books under review, the author speaks frankly and wisely on a number of awkward problems that bedevil historians today. For example, she neatly expresses our common angst at "not being able to keep up with one's reading," suggesting that it is in part the great burden of constantly encountering not the mindless trivia that anti-academics perceive but the growing stacks of challenging, well-wrought, and pertinent books and articles that discourages fresh examinations of premises and procedures.

Despite the overall excellence of the essay, however, several interrelated features were perplexing. In an essay otherwise notable for its careful distinctions and subtle judgments, Limerick early on in a simplistic and misleading fashion addresses the question of the use of the first person singular by historians. "There are . . . ," she warns, "still a significant number of historians who find the use of the first-person pronoun to be grounds for revoking a scholar's license" (122). I think she means that those of us who resist the injection of the first-person pronoun

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into historical prose are sort of fuddy-duddies, but I am not sure I picked up on her tone here. Jocularity aside, and whatever the private and no doubt irrational prejudices of some people, there are serious issues involved with the decision to inject the first-person pronoun into historical writing.

In the next sentence, Limerick appears to equate principled avoidance of the first-person pronoun with advocacy of "neutral, objective, bias-less, and passion-free history." But these four terms are hardly synonymous and do not carry the weight of disdain she seems to intend. Objectivity in a certain sense, for example, would seem to be a goal for all historians, indeed for all serious people, in that investigators should strive to collect and gather all relevant information, refrain from conscious distortion of evidence, consider contrary interpretations fairly, and routinely practice a certain degree of self-scrutiny in their work. All this has nothing to do with pronouns.

As for neutrality as a virtue or vice, we should be careful not to reject it out of hand. A stance of moral neutrality before questions of historical interest has little to commend it. Neutrality as to the significance or reliability of evidence, however, seems appropriate. Limerick's wise injunctions at the end of her essay that historians need to be open to surprises, constantly to question their assumptions, and to make "peace with life in the midst of contested turf" seem to me fully consonant with this sort of neutrality before the evidence. A better term might be organic or reflexive skepticism, or simple intellectual curiosity, no holds barred.

As for bias and passion, I am in favor of the latter, against the former, at least in what I take to be the normal use of these terms. Bias seems to me to involve conscious or at least unexamined distortion of perspective and perhaps manipulation of evidence in such a way as to privilege particular views or perspectives. Passion, on the other hand, especially when accompanied by other qualities, such as a sense of humor, an openness to anomaly, a willingness to recognize irony, seems to me a necessary quality for historians and, again, for all serious people.

But none of these attributes or qualities has anything to do with pronouns. The objection of even passionate historians to the use of the first-person pronoun has no connection with the straw man of colorless, disengaged, and supposedly value-free history. It has to do with the coherence of historical writing, with the integrity of the past that the historian is seeking to understand, and with the avoidance of self-indulgence and distraction. Rare is the scholarly book, regardless of the rhetorical strategy adopted in the text, that does not contain copious information about the author. Dust jackets display pictures and captions. Prefaces constitute a veritable diary. Introductions reveal the inner secrets of psyche and motivation, the passions and biases, if you will, of the scholar. By the time we reach the text, we know about the author's gender, marital status, geographical peregrinations, class and ethnic background, politics, education, friendships, hobbies, quirks, and even sports team loyalties. Surely any intelligent reader is able to carry this knowledge into the text itself. Surely she need not be confronted every second paragraph with the stark "I" that injects the late twentieth-century personage of the author anachronistically into, say, a mid-nineteenth-century missionary household in Hawaii.

There is, of course, a case to be made for I. The I is so important, it is argued, that it must constantly be made to confront and even affront the reader. History is constructed; its substance depends utterly on who is doing the constructing; therefore, not to pepper the page with I's is to be dishonest and evasive. The I, in short, is the author's insurance that the reader will not forget, even for one minute, that what she now reads is an arbitrary, ephemeral, constructed text that has nothing to do with History and everything to do with other kinds of purposes.

Fair enough. Although it does seem to me that the I's gracing the pages of the journal articles, monographs, journal submissions, and book manuscripts that I regularly encounter do not generally seem to have this sort of rationale behind them. Rather, they seem arbitrary interjections, careless evidences of inconsistency. Authorial personification typically does not systematically infuse the article or manuscript; instead, it appears at random, arbitrarily, often amid ponderous pseudo-scientific prose that seems to aim not for constant agitation of the reader but rather for the acquiescence that comes from boredom. Maybe there is a method even in this use of I. Maybe randomness is exactly the point. Defamiliarize. Disconcert. Be arbitrary and unpredictable. Use authorial presence as a kind of poltergeist, keeping the reader off balance without committing yourself to the full I agenda (see the paragraph immediately above). If so, I have been had, and, alas, not for the first time.

For my part, however, *pace* Peter Novick, most of the history I have been reading for the past thirty years has been written (and is certainly read) in a context that partakes powerfully of contingency, partialness, tentativeness. John Dewey, Carl Becker, and Charles A. Beard did not write for nothing. Perhaps I have been precocious; perhaps others, not as intellectually agile as I, need the I to remind them. Yes, that *must* be it.

I have no desire to revoke anyone's license. I have only three constricted arenas in which I can practice my anti-Iery: in my own prose; in that of graduate students so incautious as to wander my way; and in my comments to journals, presses, and prize committees that sometimes seek, but rarely follow, my advice. Still, the injustices in Limerick's otherwise excellent essay do need response. The bottom line is that you can be—indeed, should be—a passionate, engaged,

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and eminently multicultural historian while leaving I where it belongs, namely in the preface.

ROBERT H. ZIEGER
University of Florida, Gainesville

Patricia Limerick does not wish to respond.

THE EDITOR

TO THE EDITOR:

Should Dominick LaCapra have the final word in his exchange with Russell Jacoby on the "New Intellectual History" (*AHR*, 97 [April 1992]: 405–39)? Although I am in no position to join, much less to conclude, that conversation, I am perhaps better situated to comment on Jacoby's apparently "massive misconstruction of [LaCapra's] 'On Grubbing in My Personal Archives: An Historical Exposé of Sorts'" (432). That piece, as Jacoby indicates, challenges my own views of recent developments in intellectual history, including LaCapra's work (422).

I wish to raise two closely related issues. The first concerns the genre that LaCapra has claimed in his earlier text (*Boundary 2*, 13 [1985]: 43–67), an important consideration here because genre clearly informs interpretation. Is this piece truly what the author calls it, a parody? In light of the obvious hyperbole from the title onward, the answer must be yes. But it is also something else: it is a polemic (like this letter) and can be read as such. LaCapra admits that he is arguing as well as exaggerating. And so he too quotes freely from this particular essay, in his personal correspondence and elsewhere, as if it were mere argumentation. LaCapra himself treats this parody as polemic. It would seem then that Jacoby's deliberate focus on one ostensible feature in the text is no more massive a misconstruction than LaCapra's own.

The other issue concerns the contextual interpretation of this piece. Its author writes, "One may want to take issue with my claims or the style in which they are conveyed, but one must see them in the context of an essay that interrelates argument with more indirect modes of address such as irony, parody, and hyperbole" (*AHR*, 432). But that is certainly not the only meaningful location for LaCapra's earlier article. At the level of the text itself, there is its participation in generic and stylistic traditions, of parody and polemic especially, as noted above. And, at the level of discourse, there is its place among other texts that influence response. These other voices include the objects of LaCapra's ridicule, of course; but they also exist, implicitly at least, in the minds of both author and reader.

For example, what makes LaCapra's gloss on Jacoby so intriguing to me arises in one discursive situation in particular, that of my personal correspondence with both men about LaCapra's "On Grubbing in My Personal Archives." In November 1990, Jacoby wrote to me requesting my reflections on LaCapra's essay in *Boundary 2*, evidently for the purpose of writing what finally appeared in the *AHR*. I responded frankly and identified serious distortions and omissions in LaCapra's original account of our exchange in 1983. For LaCapra's information, I sent him a copy. He then wrote to me (cc. Jacoby) to claim the position he ultimately took in the *AHR Forum* (that his piece in *Boundary 2* was something of a spoof). It is this context, then, that explains, I think, LaCapra's subsequent reference to Jacoby's "manifest inclination to identify with the putative underdog . . . in straightforward if not storybook terms" (*AHR*, 432). The story to which LaCapra alludes is not so much in Jacoby's *AHR* article as it is in an unacknowledged subtext: my letters of two years ago.

One may deduce from these considerations that the issues LaCapra raises, like his parodic-cum-polemical prose, are more uncanny than he thinks. They are also, as Jacoby argues, more formalistic than he admits. In his *AHR* essay, LaCapra still prefers text to context, synchronism to diachronism, and critique to scholarship, preferences that would substantially redefine important features of our discipline if widely adopted. His ideas even treat the historian's traditional use of evidence as a neopositivistic anachronism, such as my own work in the history of reading (LaCapra, *History and Criticism* [1985], 129–30). For that theoretical admonition, if for nothing else, encountering the author has been revealing. But I do not recommend it to just anyone, certainly not to scholars in the bush leagues, where, according to LaCapra, historians do not speak for themselves. In the great American desert west of Ithaca, lesser members of our profession apparently all chime with self-certain authority, like a chorus of angels, in "the hegemonic voice of historiography" (*Boundary 2*, 49). Indeed. However clever, such theory makes very curious history.

JAMES SMITH ALLEN
Southern Illinois University, Carbondale

DOMINICK LACAPRA REPLIES:

I doubt if many historians would have even heard of my essay, "On Grubbing in My Personal Archives: An Historical Exposé of Sorts (or How I Learned to Stop Worrying and Love Transference)," if it had not been for Russell Jacoby's article. I do not "quote freely" from it; in fact, before my response to Jacoby, I had never in my published work even referred to it, and I mentioned it in personal correspondence only in direct response to the claims of James Smith Allen. As should have been evident from my remarks in the *AHR*, I myself have divided feelings about the essay. But I never intimated that it was merely "something of a spoof." As indicated in the passage Allen quotes, I see it as a mixed generic performance "that interrelates argument with more indirect modes of ad-

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dress such as irony, parody, and hyperbole." Among the genres it invokes is the mock confession. Its use of polemic is subdued and moderated by self-parody. But the basic point I was trying to make in the *AHR* is that an interpretation such as Jacoby's is very misleading in that it isolates one dimension, gives no indication whatsoever of the essay's other dimensions that have a crucial bearing on understanding it, and repositions the one dimension it isolates in a simplistic scenario of its own fabrication (involving such media-hype terminology as "bush leagues" and "back down").

The "straightforward if not storybook" nature of Jacoby's rendition requires no subtext; it is blatantly obvious in his contribution to the *AHR*. I would add that I did not agree that there were "serious omissions and distortions" in my "account of [Allen's and my] exchange in 1983," and I tried to show why in the letter (cc. Jacoby) to which Allen refers. Jacoby's mind was apparently made up, and the first I heard of his "research" was through Allen. Jacoby never contacted me to solicit my view of the matter and sent only a curt acknowledgement of the letter I sent him after I received the letter Allen was good enough to share with me.

The more basic points raised by Allen come in his final paragraph. I certainly insist that the work of different historians will inevitably and justifiably reveal different stresses and strains. And, while recognizing the need for a flexible consensus on disciplinary and professional norms, I feel that it is misleading to think that there can be one model of "correct" history to which all historians should conform. But I do not see how my *AHR* essay could lead to the impression that I prefer "text to context, synchronism to diachronism, and critique to scholarship." Both in that essay and throughout my work, I have tried to pose and explore the relationships among these phenomena—relationships that I have attempted to interpret in interactive rather than simple binary terms. However, I have never denied the force and effectiveness of binarism in texts or in social practice. In fact, a facile "undoing" of binaries often induces a blindness to their actual role and can have misleadingly apologetic functions. (In *Soundings in Critical Theory* and elsewhere, I have argued this point, for example, with respect to Derrida's strained attempt to read Paul de Man's wartime journalistic piece, "Les juifs dans la littérature actuelle"—which relies massively on binaries and stereotypes—as if it were strongly self-questioning and self-deconstructive.) Allen's own formulation is one more piece of evidence attesting to the force and effectiveness of binary oppositions, for he converts back into binarism what is clearly in my work an attempt to analyze critically binarism's empirical role and to question certain of its effects (notably in the establishment of rigid hierarchies and in processes of scapegoating and victimization).

I have tended to stress the problem of critical reading and the need to inquire carefully into the question of precisely how texts come to terms with various contexts because I think that these issues have tended to be underemphasized or resolved in overly restricted ways in professional historiography. But this stress never implied the unimportance of contexts, nor was it ever accompanied by a synchronic perspective. I would agree with Allen that I (with others) have advocated certain changes in the historical profession—changes that have to some significant extent been effected in the recent past. In my work, these changes never involved the abandonment of the traditional use of evidence, although they did—and do—involve doubts about neopositivism, for example, in the form of a history of reception that restricts itself to chronicling the readings of others without attending to the position of the historian or the problem of a critical reading of texts. The most important point, however, is that I would very much defend a conception of research that is not restricted to scholarship in the conventional sense but includes questions of theory and critique. To say this is in no sense to denigrate the significance of scholarship; it is to insist that scholarship be explicitly accompanied by critical forms of inquiry that can only enhance its value and help prevent its decline into ideology and uninformed polemic.

DOMINICK LACAPRA
Cornell University

REVIEWS OF BOOKS

TO THE EDITOR:

It is always saddening to see one's work misread or misrepresented within the compass of a brief review. But, rather than attempt to respond to every point raised in Elizabeth Garber's negative notice of my *Questions of Power: Electricity and Environment in Inter-War Britain* (*AHR*, 96 [December 1991]: 1549–51), I would like to restrict myself to a small number of comments on her interpretation of the second half of my book.

The unwitting reader might be led to believe that my stance is that of a naïve preservationist, uncritically supportive of the anti-technological ideology of those intellectuals, "aesthetes," and communities that so stridently opposed the "march of the pylons" in interwar Britain. Nothing could be further from the truth. Indeed, no fewer than eighty pages of *Questions of Power* are devoted to a critical analysis of the differing motives and social interests of those who argued that the costs of electrification would outweigh the benefits, and of the extent to which "a specific sense of social insecurity was frequently and unambiguously sublimated into the protection of an arcadia under threat" (168). Unlike some of the crankier members of the "anti-electrical" lobby at that

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time, I did not question the inevitability of the large-scale adoption of the new source of energy. Rather, I sought to demonstrate how the state administration undermined the public inquiry system in such a way as to marginalize "anti-progressive," "anti-electrical" views before they could be fully articulated. Far from being vitiated by the naïve culturalism adduced by Garber, this section of my book is openly, deliberately, and extensively socio-political.

As for the afterword on the "atomic age," this was not, as Garber wrongly suggests, predominantly concerned with either lamenting the destruction of an "unspoilt England" or with the contemporary politics of privatization. I am deeply interested in and concerned about both these issues, but the central purpose of the chapter was to delineate a distinctive phase of environmental thinking, intermediate between classic interwar conservationism and the "new ecology" of the later 1960s. The chapter also sought to show how televisual representation has yet further enhanced the seductiveness of new and subtly subliminal forms of technological triumphalism.

Finally, and crucially, Garber is hostile to the entire social historical ethos that underlies *Questions of Power*. She naïvely believes that it is possible unproblematically to discern two "worlds"—large-scale, "objective" technological systems and the public "reactions" associated with them. She also confidently posits a clear-cut differentiation between new technologies and their implementation. My own view is that such dichotomies are not nearly as clear cut—historically—as "theory" might suggest. Hence, in part, the heavy emphasis in the first part of *Questions of Power* on rhetoric, discourse, and imagery—an approach Garber is determined to condemn rather than argue with or critically evaluate. I hope that other readers will be more open minded. More specifically, I am confident that even a casual glance at *Questions of Power* would show that Garber's intemperate accusations of propagandizing and lack of historical integrity are unfounded. These are serious charges and cannot be easily refuted. But they should only rarely be given public currency, and certainly not by a reviewer who is convinced that an author should have written a quite different type of book.

BILL LUCKIN
Bolton Institute of Higher Education, United Kingdom

ELIZABETH GARBER REPLIES:

The categories in which Bill Luckin situates me and into which he claims I push him are irrelevant. The focus should be his text. That the actual forms of technological systems are socially, politically, and culturally negotiated is a given. Whether his monograph has sufficiently discussed and explained those negotiations that brought electricity into the domestic sphere in Britain is another matter.

Luckin claims to examine the process, not judge the outcome. To do this, he focuses on the rhetoric, in certain cases, in the debates over pricing structures in parts of Manchester, the siting of the Battersea power plant, and, more extensively, the route of the National Grid.

To detail the process and reveal the negotiated aspects, he needs to define the technological system of providing electricity: it must be generated, transmitted, then used, no matter what form the actual technology takes. This he does not do. He also needs to define the system as it appeared in its final form from government planners and the industry before negotiations began in the public sphere. This he does not do. The text is on public negotiations, not those inside government or industry, but context is missing here that is necessary to understand the debate. He needs to delineate which aspects of the system in the British context were politically, socially, and culturally contested and contestable. Finally, Luckin should tell his readers why the particular cases he chose are so illustrative of the process. Clearly, the text focuses on and interprets the contested aspects of the system.

I do not think a discussion of propaganda and advertising materials from the Electricity Board describing the wonders of electricity and its uses in the home is sufficient to answer the needs of defining the system, especially since the debates themselves are not over electricity and "what it can do for you" but over how electricity will be transmitted to rural and urban consumers. If judgments on transmission are judgments on the whole system, using "electricity" in the sense delineated in the text, some connective tissue is necessary, theoretical or evidential. In his discussion on the Battersea power plant siting, Luckin details objections on the grounds of public health and the preservation of public monuments. To claim that those who opposed the siting saw electricity as an "alien presence which would 'corrode' the entire social fabric" flies in the face of the evidence, unless some theoretical net is cast from text to subtext. A sentence on Mary Douglas is suggestive but does not constitute such a theory. Unsubstantiated claims involving unwarranted inferences will no longer suffice to situate technology in Western society.

In the debate over the continuing health of men and monuments, Luckin does not connect the rhetoric to the actual state of knowledge, negotiated though that may be, of both the corrosive effects of sulphurous oxides and available methods of alleviation. The rhetoric is left to represent reality as well as perceived reality, without distinguishing the two.

There are other examples of dubious connections discussed in my review. As I stated there, electrification is an important subject in and of itself, and Luckin's is one of the first monographs on the topic for Britain. The necessity to examine the coming of electricity in the twentieth century needs no further comment. But any such examination should reveal those aspects of technological systems that situate it in a particular context, i.e., put technology into history.

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To do this requires explicit theoretical tissues that are evidentially plausible rather than generalized theoretical commitments that lead to arbitrary couplings. The latter only tell the reader about the political, social, and cultural commitments of late twentieth-century historians rather than shed light on the historical process under discussion. Luckin's afterword on nuclear power is neither mentioned in his introduction nor connected to the subject of the main chapters of his monograph. How it fits into his discussion therefore remains up to the readers to discern for themselves.

ELIZABETH GARBER
State University of New York at Stony Brook

TO THE EDITOR:

Reviewing Jeremy Wilson's *Lawrence of Arabia: The Authorized Biography of T. E. Lawrence*, in the *AHR*, 96 (December 1991): 1587–88, Janice J. Terry has chosen to highlight, in her brief notice of a very large book, some short and perfunctory remarks about some writings of mine made by the author.

She reports, and seemingly applauds, Wilson's opinion that my "studies on Arab nationalism, the conquest of Damascus, and British policies are weakened, if not considerably undermined," by my emphasis on reports from officials in India over those from Cairo. I am puzzled by this statement. Historians are in duty bound to look at all the evidence whether from Cairo, India, or Samarkand. Their business is not automatically to dismiss evidence because it comes from a particular source and privilege other evidence because it comes from another. Rather, it is to examine any particular piece of evidence and come to their own judgment about its credibility and cogency in the light of all the evidence at their disposal. I am not aware that I have proceeded otherwise in my writings, or that I have, specifically, preferred evidence emanating from "officials in India" over evidence from elsewhere because it came from that source. The "authorized biographer" produced no argument or evidence to the contrary, and neither does your reviewer.

Regarding the conquest of Damascus, your reviewer, copying the allegations of her author, declares that my study of this episode was written before all the documents were made available. This is not so. The episode was examined in a book published in 1956 and was reexamined in the light of the documents made available in 1968 in an essay included in a book published in 1970. The new evidence confirms and amplifies my account of 1956. In 1990, the "authorized biographer" finds nothing to add to the story, which is, briefly, that General Allenby forbade all his troops, with the exception of the so-called Northern Arab Army, to enter Damascus, and that this was followed by general disorder and looting. Nor does Wilson deny that Lawrence confessed in a passage in the manuscript of *Seven Pillars of Wisdom* that the Sharifians were deliberately allowed to occupy Damascus—a passage Lawrence then suppressed in the published version (1926). Had it not been that Lawrence's brother and literary executor fiercely stood guard for decades over his brother's papers, we would not have had to wait so long for this confession—which concerns a crucial issue in the modern history of the Middle East—to be released to the public.

It is very much to be desired that the knowledge of those who accept the responsibility of reviewing for a learned journal should extend to somewhat more than what they can glean from the book they are reviewing.

ELIE KEDOURIE
Woodrow Wilson Center, Washington, D.C.

JANICE TERRY REPLIES:

Elie Kedourie has taken offense where none was intended. In a review limited to 1,000 words, it is clearly impossible to analyze, in the sort of detail suggested, a 1,000-plus page book. Given this constraint, I chose to provide readers, not with an abbreviated version of my own research in the relevant Public Record documents and British-held collections, but with a summary highlighting Jeremy Wilson's interpretations and descriptions of the controversial Lawrence. The debate over the events surrounding the conquest of Damascus continues—witness this correspondence. I leave it up to Wilson and Kedourie, if they so choose, to marshal the evidence and to make the arguments regarding Lawrence's role in the events preceding and following the final days of the war in greater Syria.

As historians researching Arab nationalism, the Arab revolt, and Lawrence's role in it continue to focus their attention on issues revolving around the internal machinations of the British Foreign Office and government, more important and crucial questions remain largely unaddressed. To whit, how did and do Arab nationalists view Lawrence? Were he and the other British "advisers" crucial to the decisions taken in the conduct of the Arab struggle against the Ottoman Turks? Did Lawrence and others purposely mislead and obfuscate British intentions regarding the future of the Arab world? To repeat the conclusion of the original review, these questions must be answered before Lawrence's role in the Arab revolt and his long-term impact—if any—on the Arab world can be put in proper perspective.

JANICE J. TERRY
Eastern Michigan University

TO THE EDITOR:

In his intemperate review of my book, *The Chinese Revolution in Historical Perspective* (*AHR*, 97 [February

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1992]: 264), Arif Dirlik deems it a "reckless" example of "a recent trend" in Chinese studies "that has been fatuously described by its proponents as 'Chinese history from the inside.'" Dirlik's views are his own business, but surely reviewing a book that represents a new approach—particularly one that a person strongly disagrees with—calls for a precise and dispassionate analysis. Instead, Dirlik has written a vituperative polemic. Readers, I am certain, can draw their own conclusions about the scholarly reliability of such a "review." I cannot here answer all of Dirlik's angry accusations, but I wish to focus on two issues.

The first is to defend this "recent trend" that so provokes Dirlik. The approach has many aspects: analyzing Chinese history in its own terms, highlighting the relationship between nineteenth and twentieth-century China and the past, and working to reintegrate the West into the modern story while keeping the Sinocentric focus. I am proud that my work suggests new and interesting answers to these questions. Dirlik's flat statement that the book "does not differ significantly from existing interpretations" is not only wrong but seems designed to trivialize the work in the minds of prospective readers. His assertion stands in marked contrast to all other comments on the work, whether in agreement with my ideas or not.

Dirlik's specific disagreement with the new trend appears to be that Marxist historiography has been dominant in China for seventy years and that China is now a Communist country; therefore any approach that takes fundamental issue with the Marxist historical perspectives propounded there is *ipso facto* a highly Western perspective and, indeed, hegemonic (his word, not mine). I am not convinced. Many scholars in the West, in Taiwan, and increasingly (though quietly) in China itself believe that Communism has cut China off from many of its most enduring and valuable traditions, including Confucianism. Whatever one's views, the issues involved here are surely open to legitimate, cool-headed discussion. Can Dirlik think otherwise?

Although Dirlik's diatribe forces me to respond sharply here, I should note that my book is far from a political tract. I employ Confucian social theories carefully, calmly, and, I think, objectively in analyzing—among other things—the similarities and differences between Chinese Communism and traditional radicalism. I certainly do not resort to "culturalist reproach" without analysis, as Dirlik asserts, and a reviewer thus responding seems rather to betray his own fear of any deviation from a preconceived orthodoxy.

Secondly, I want to emphasize that Dirlik is in serious factual error when he writes that I do not understand my key terms, *fengjian* and *junxian*. He brushes aside the two words—among the most basic categories of historical analysis used in East Asia before the impact of the West—as simply "administrative terms" and claims that I arbitrarily give them a richer than justified panoply of associations akin to "Marxist notions of 'mode of production' or 'social formation.'" Can Dirlik truly believe that before Marxism, China was unable to produce rich socio-political concepts? His uninformed and unsupported claim is tantamount to arguing that a complex Western term such as "liberalism" can be fully understood merely through a dictionary definition.

Dirlik is equally off the mark when he asserts that "neither the concepts nor their relationship to Chinese history is rigorously analyzed." When the key terms first appear in the preface and again on page 4, the reader is directed to a two-page note providing a careful analysis of their traditional meaning and historical associations. In fact, the eminent sinologist Yang Lien-sheng had already written about them, and I was honored to build on his work and to have his support and assistance during my own research. After their introduction, I return to the terms time and again, elucidating them further while relating them to the entire course of Chinese history. On this score as on many others, I am hard pressed to recognize the book Dirlik has reviewed as mine.

JOHN SCHRECKER
Brandeis University

ARIF DIRLIK REPLIES:

John Schrecker's response to my review of his book confirms what I said in the review: he has little to say of substantial intellectual or historiographical interest—and if he does, has yet to find a way to say it effectively. Schrecker is entitled to any defense of his work that he wishes; contrary to his impression, I have no wish to engage him in polemics. He seems to miss the point of my criticisms on one or two issues.

With reference to the concepts that are central to his analysis, *fengjian* and *junxian*, Schrecker writes: "Can Dirlik truly believe that before Marxism, China was unable to produce rich socio-political concepts?" I am not sure if this question is directed at me as writer of the review or some Dirlik phantom in Schrecker's imagination. First, I described the concepts as "administrative" concepts, based on the same authorities that he cites and said nothing about their wealth or poverty. If Schrecker thinks that concepts must be lacking in richness because they are "administrative," that is his problem, not mine. The "administrativeness," in shaping relations between state and society, is in my opinion quite significant in its implications. The concepts are not rich or poor; they simply point to a different conceptualization of society and politics and of the relationship between the two. Second, I did not charge Schrecker with "misunderstanding" but with *reinterpreting* the concepts—quite a difference, I should think, in directing attention to the historicity of the concepts, including that in his usage. Third, I did not say anything in the review about Chinese capabilities "before Marxism" but only that in Schrecker's interpretation, the concepts appear as

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something akin to Marxist notions of mode of production, which enables him to turn around and apply them globally. Again, quite a difference. What he is doing, as an intellectual operation, is not different from what Chinese Communist historians (not the same thing as "Marxists") have done with such concepts as "feudalism." In this regard, Schrecker should be well aware that the rewriting of Chinese history did not begin with Marxism but in the Chinese nationalist confrontation with Euro-America. If he wishes to single out Marxism through some preoccupation that is his, I cannot do much about it. It may be noteworthy, however, in a work that makes such grandiose claims, that much of the literature dealing with Chinese reconceptualization/rewriting of history in the twentieth century is missing from his bibliography—and his discussion.

Rejecting the imposition of a Euro-American historical narrative on China's past, Schrecker turns around to rewrite Chinese and Euro-American histories in another unilinear narrative based on two Chinese concepts. He, therefore, seems to miss the point about my objection, which is to the unilinearity of global historical narratives, whether European or Chinese in the concepts informing them. This impulse is itself very modern, and of Euro-American origin, which calls into question his very undertaking.

I am not surprised that Schrecker does not "recognize the book Dirlik has reviewed as" his—since he seems to be unreflective about the way he uses historical concepts and the ideological premises of his historical interpretation, not to speak of the enormity of the cultural arrogance he displays in stating that the Chinese themselves have lost touch with the dynamics of their own past. He may or may not be aware that there is more than one way to be in touch with the past and that new concepts put us in touch with the past in new ways. In seeking to put the Chinese back in touch with their past the way he understands it, he may have an agenda of his own that remains unexplained in the book. The Chinese certainly have been in touch with their past(s) in diverse ways, ways that Schrecker does not seem to approve of. He might just wish to consult some of the rather extensive literature that exists, for China and the non-Euro-American world in general, on the intellectual problems and agonizing questions that have been raised in those societies by the confrontation between modernity and their various pasts. Since his work does not raise any serious questions in this regard, it gives the reader the unambiguous impression, whether or not so intended, that it is up to the "orientalists" in Euro-American universities to put the natives back in touch with their pasts, which they, the natives, have lost because they have become too contemporary or too enamored of the concepts of modernity—in this case, I suppose, modernity informed by Marxism.

ARIF DIRLIK
Duke University

TO THE EDITOR:

David Kopf in his review of my book, *Brothers against the Raj: A Biography of Indian Nationalists Sarat and Subhas Bose* (*AHR*, 97 [February 1992]: 270–71), wishes that I had written two different books rather than the one I have written. He does not fulfill his obligation as a reviewer to describe and assess the book I did write.

I have written the biography of two Indian leaders, placing them in the context of a developing nationalist movement and analyzing their role in the Bengal Provincial Congress, the Indian National Congress, the Calcutta Corporation, the Bengal Legislative Assembly, in the Indian Left, and in Hindu-Muslim relations. I believe that I have made a contribution to our understanding of all these institutions and relationships and of the leader of the Congress, Mahatma Gandhi. This is a book about Indians, and 85 percent of it is set in India. None of these specifics is touched on at all in the review, although Kopf says he did not get the general treatment of Indian nationalism he would have liked. Indeed, it is not a general theory of nationalism or a general history. For the former, he would be better served by the works of Benedict Anderson or Partha Chatterjee on nationalism. However, not to mention what one might learn about Gandhi and the Congress through my study of the Boses does seem off the mark.

The second book that Kopf would have liked was a diatribe against the Nazis and a study of the Holocaust. Subhas Bose, as I have written in my book, made a pact with the devil, Nazism, and spent the period April 1941 to February 1943 in Europe. He had hoped to go to Japan or the Soviet Union in search of external assistance for Indian nationalism, but neither of these powers wanted him. So he went to Germany. From 1933 onward, he had protested against Nazi racism and brutality; but to free his country, not because he was sympathetic to Nazism, he decided to go to Germany. I think it is very clear in the account that I have given that he was unhappy, frustrated, and angry throughout this period, and I see it as the worst mistake of his life.

My personal feelings are made clear in the prologue, where I explain how difficult it was for me as a Jewish person to go to Germany and interview some of the former Nazis who helped to train Bose's military force. Even though my antipathy to the Nazis runs through this chapter, Kopf wants more. He says that Hitler killed 250,000 "Indians," that is, the gypsies, and did other dastardly things. I believe I have carefully and dramatically presented the story showing the wickedness of the Nazis where relevant to Bose's life.

For example, I have told the story of Bose and his Foreign Office contact in Germany, Adam von Trott zu Solz. Trott used the Indian connection to help Bose and also as a cover for his work on the assassination plot against Hitler. Trott was caught, tried,

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and executed when the plot failed. One of the tributes to my account of this matter is from the widow of Trott, Clarita von Trott, who wrote to me on August 20, 1991, "It is beyond . . . what I could have hoped for your understanding and appreciation of my husband . . . the fairness of your account, the sympathy with which you state the inevitable reservations on both sides and with which, later, you tell about Adam's fate, that has been very moving for me."

The name of Trott and even the name of Mahatma Gandhi do not enter in Kopf's review. Sarat Bose, one of the two subjects of the book, is missing. Kopf has his own idiosyncratic agenda and an unwillingness to learn about India (or Germany). He gives us someone called "Narendranath Vivekananda" who influenced Bose instead of either correct form: Narendranath Dutt or Swami Vivekananda. This is typical of the way Kopf plays fast and loose with Indians and Indian history. But for those of us who like comic relief, he has given us a new character worthy to stand with an earlier creation of his, Mr. Jessie Bose (his unique way of referring to the distinguished Bengali Jagadish Chandra Bose). But, unfortunately for your readers and for me, he has not seen fit to review the book at hand.

LEONARD A. GORDON
Columbia University and Brooklyn College

DAVID KOPF REPLIES:

Restraint and a sense of decency explain my reluctance to describe and assess Leonard Gordon's treatment of Bose in the context of the Calcutta Corporation, the Bengal Legislative Assembly, etc. I have been listening to Gordon expostulate on aspects of the Indian nationalist movement since May 1966, when he lectured on "Gandhi in Bengal" for the second Bengal Studies Conference at the University of Missouri. It was the first time I had heard him discuss Bose. For almost thirty years since, he has been to the ends of the earth collecting new data. I expected a subtle and sophisticated concept of nationalism in the book that would give fresh meaning to his mountains of data on Bose's institutional context. I was profoundly disappointed. I wanted far more than the idealization of a nationalist hero. He tells me to read Anderson or Chatterjee. Why are these writers not analyzed in a chapter on nationalist historiography? And why, after so much hard work over a lifetime, is Gordon so timid about coming up with his own hypothesis on Indian nationalism?

He reprimands me for not dealing with the "Indian" portion of Bose's activities, which constitute 85 percent of the book. The truth is that the greater part of the book is not about "India" but about Bengal. Bose, whom Gordon categorically refers to as an "Indian nationalist" in the book's subtitle, was ethnically, religiously, and culturally a Bengali Hindu remembered and revered today largely by other Bengalis. It is no accident that Gordon and I are apt to distinguish Bengal from India. We were both trained as Bengal specialists at the University of Chicago, moved in some of the same circles in Calcutta, and were lucky enough to win the Watumull Prize from the American Historical Association for monographs on modern Bengali history.

Readers unfamiliar with unity and diversity in India and Pakistan may not realize that Bengal, as in the case of other regions of South Asia, has its own distinct language, culture, and history. Each of these "regional" cultures has the potential for becoming a nation-state. In 1947, for example, East Bengal, made up mostly of Muslims, became East Pakistan and remained so until 1971, when it won its independence and became the new nation of Bangladesh.

In India, in the former Soviet Union, and elsewhere in the pluralistic universe, nationalism is a complex process that cannot be explained simply as a struggle between the freedom fighters (good guys) and the imperialists (bad guys). There is a darker side more difficult to explore. India has not been immune from the darker side. Mahatma Gandhi was assassinated by a Hindu nationalist; Indira Gandhi was a victim of Sikh nationalism; Rajib Gandhi was blown up by Sri Lankan Tamil nationalists. Although Subhas was given to violence, was authoritarian, flirted with fascism, lived among Nazis in Germany, and was the willing accomplice of Japanese militarists, Gordon ignores the darker side of Bose. Typical of the sentimental rubbish that runs through the book is Gordon's final assessment of the Boses in the very last paragraph: "The Boses now live in the historical imagination of their countrymen. Bound by love and a common cause, they struggled against imperialism with great perseverance and courage . . . They should be remembered for the zest and devotion they gave to their country as they tried to fulfill their own and India's destiny."

Finally, Gordon accuses me of "playing fast and loose with Indians and Indian history" because, in the review, Vivekananda is incorrectly referred to as Narendranath Vivekananda. Also, thirteen years ago in a book on the Brahmo Samaj, I referred to Jagadish Chandra Bose as Jessie. It does not take a Sherlock Holmes to guess that in the first instance I was a victim of a word processor. Originally, it was "Narendranath Dutt or Vivekananda," but the computer, unaware of Gordon's eagle eye for minutia, deleted "Dutt or." The source for "Jessie" was Ms. Sujata Bose, granddaughter of Ananda Mohun Bose, who sent me a letter describing family documents on the Brahmo Samaj. Jagadish was referred to four times in the letter as Jessie. I used it quite innocently, not realizing that it was providing Gordon with comic relief all these years.

DAVID KOPF
University of Minnesota, Minneapolis

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TO THE EDITOR:

In a tandem review [*AHR*, 97 (April 1992): 583–84], Barbara Evans Clements did her best to give a balanced account of Robert Service's biography of Lenin and my study of Lenin, Trotsky, and Stalin. I do not think she quite succeeded in her review of my book. Her review suggests that I repeat "the old charge that the Bolsheviks were despots from the cradle" (p. 584). I cannot say who has made that charge in the past, but I certainly did not make it. Nor did I say that Lenin, Trotsky, and Stalin were genetically Bolshevik, although maybe someone has said that, too. Unfortunately, the statement quoted is in keeping with Clements' misleading portrayal of the book as ahistorical and full of oversimplifications. I must confess that I am a novice at oversimplifications compared to her, and I am not going to try to summarize a complex book here, although your readers deserved better than they got. Clements also claims that I overlook "rational and ethical motives" (whatever those words mean to her) and Lenin's cogency. I think I can shed light on my position and her review by stating the following: her review was rational and ethical, in the Leninist tradition, but I do not think she is as cogent as Lenin was.

PHILIP POMPER
Wesleyan University

Barbara Evans Clements does not wish to reply.
THE EDITOR

ERRATUM

We regret that the name of General Matthew Ridgway was misspelled in the April 1992 issue, page 496. Our thanks to Joseph Forbes of Pittsburgh for pointing out this error.

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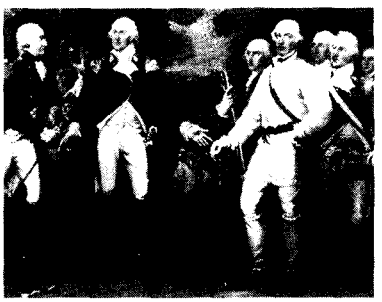
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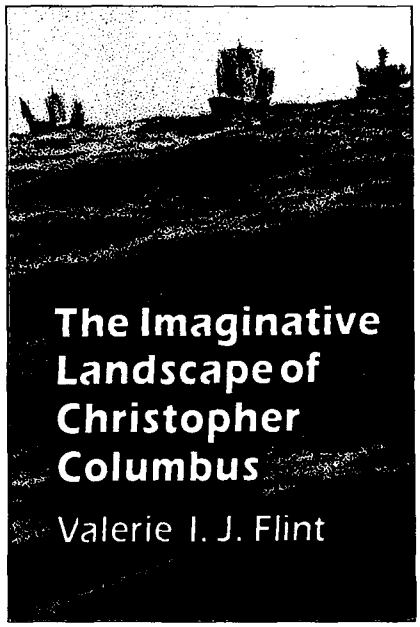
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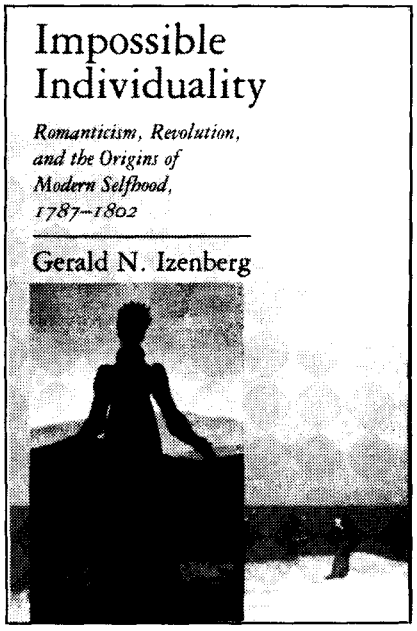
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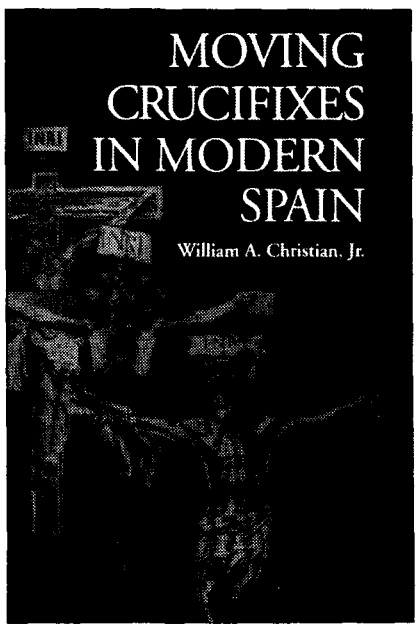
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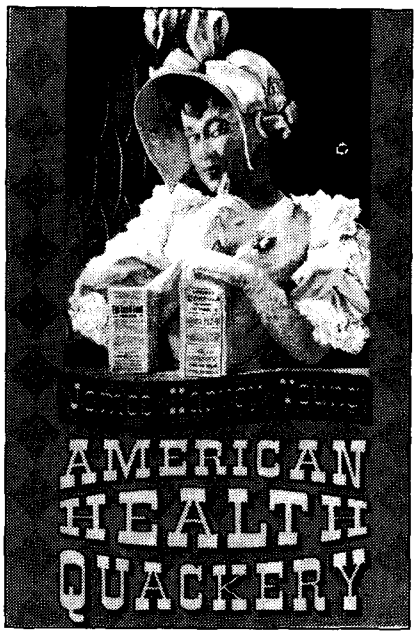
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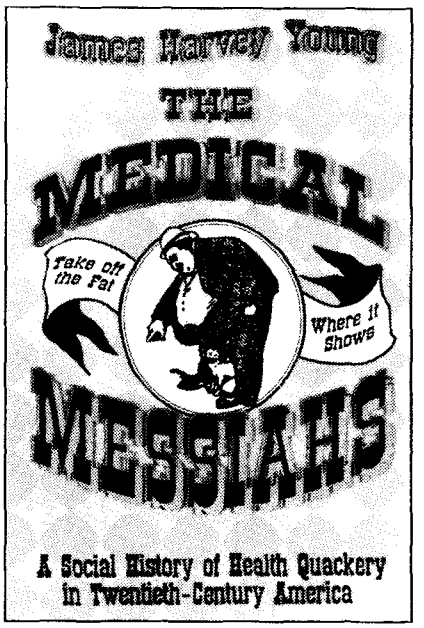
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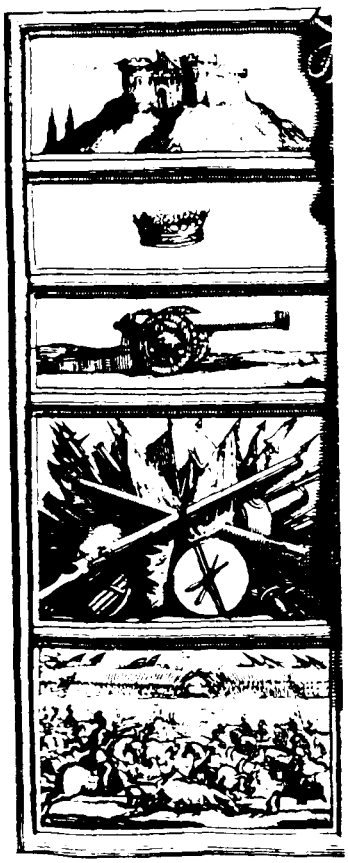
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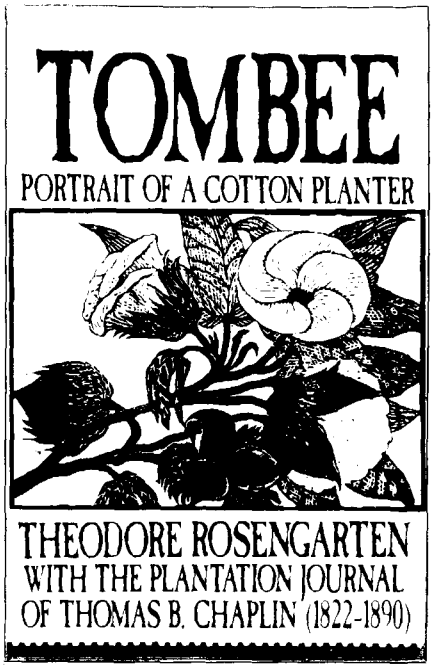
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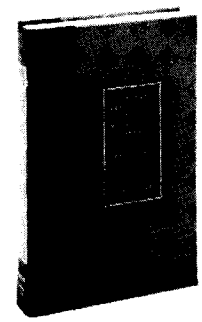
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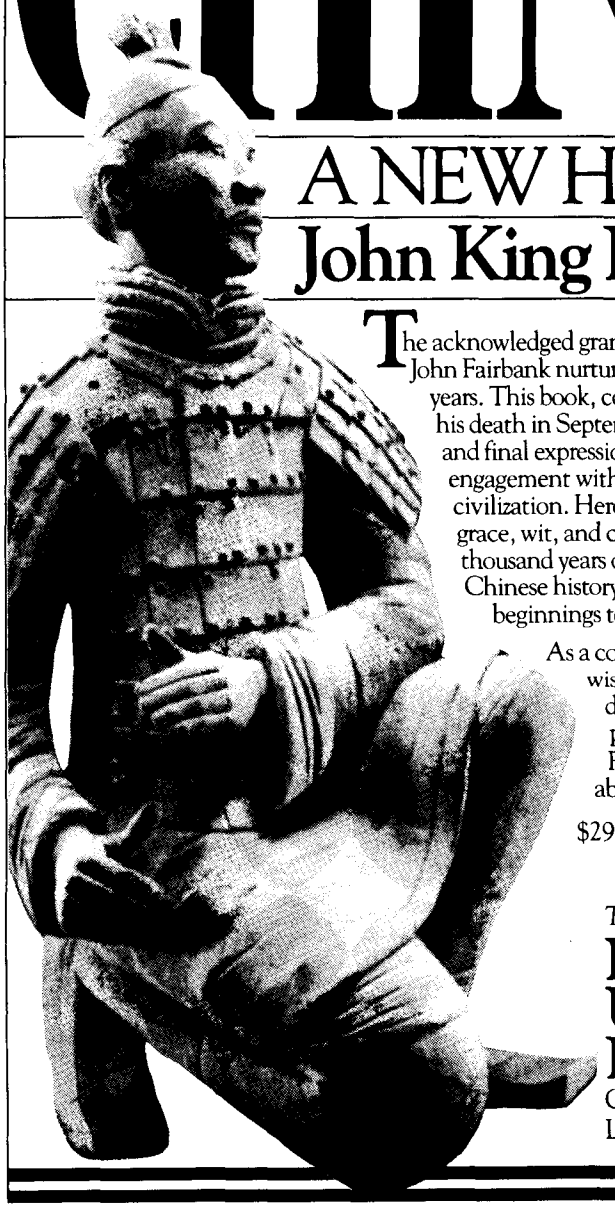
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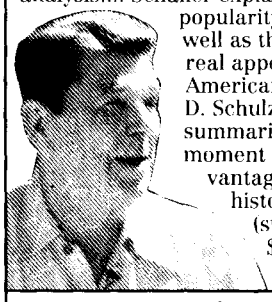
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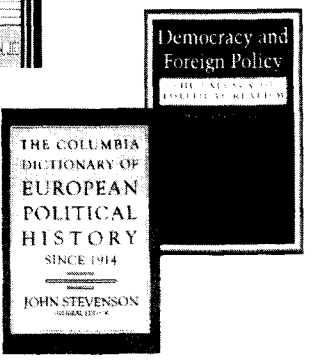
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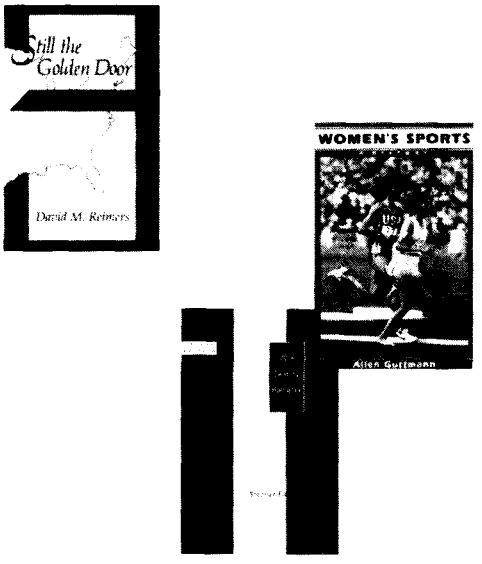
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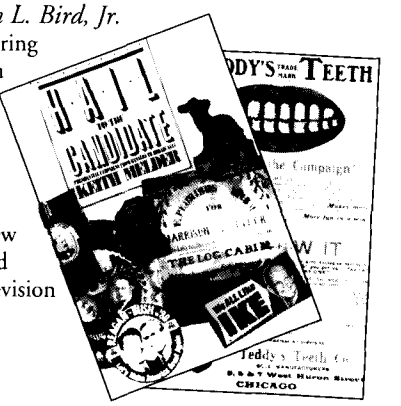
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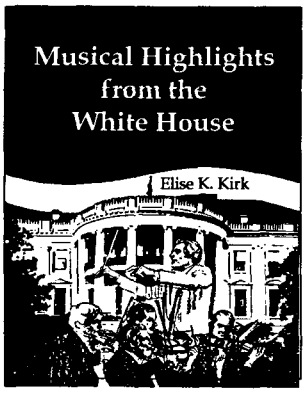
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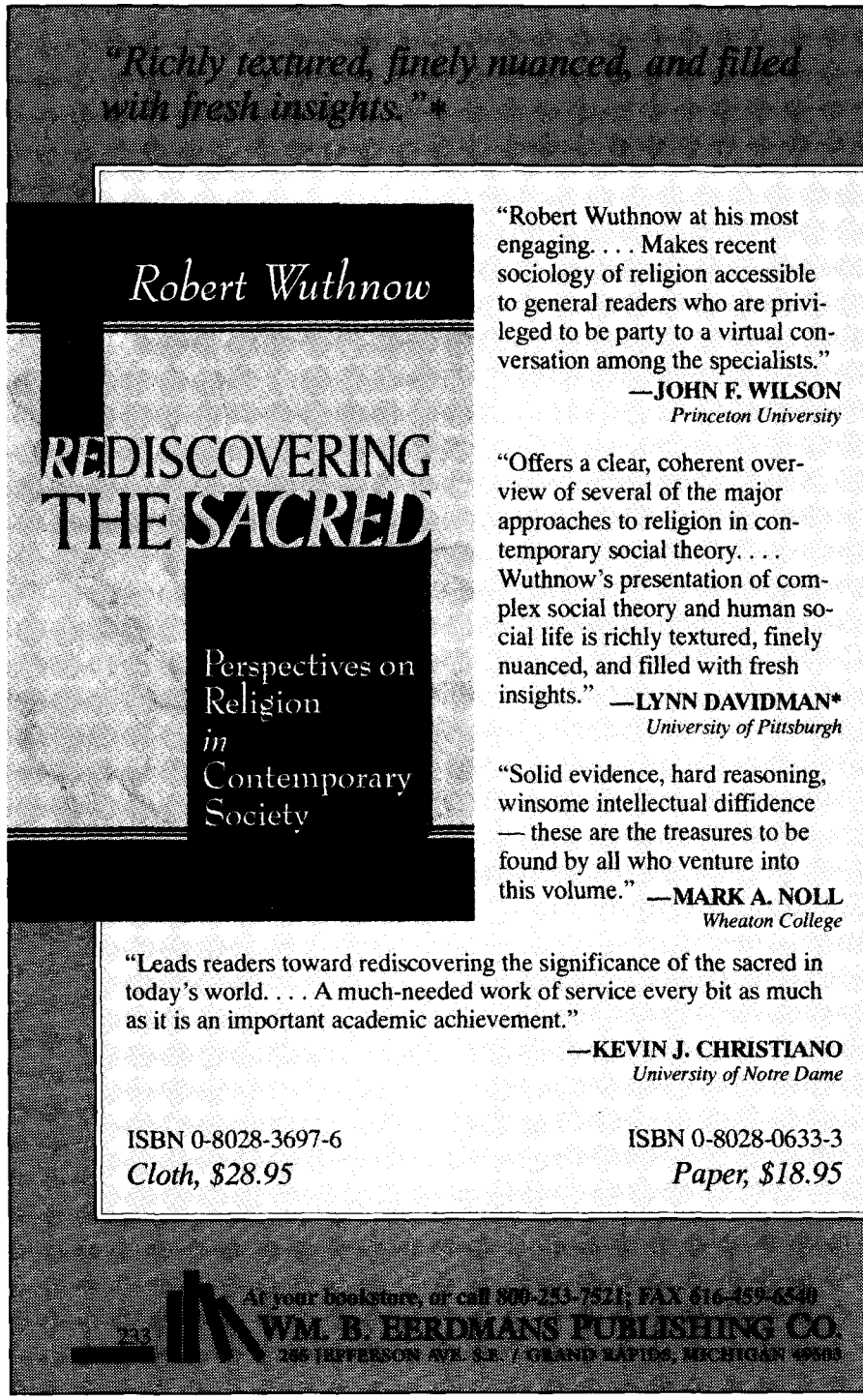
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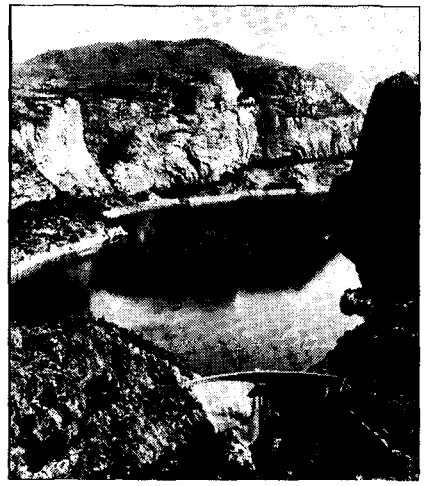
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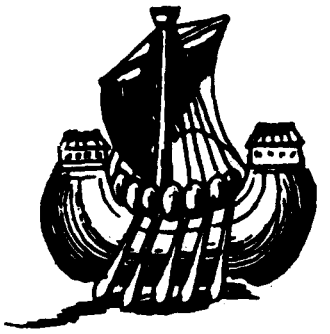
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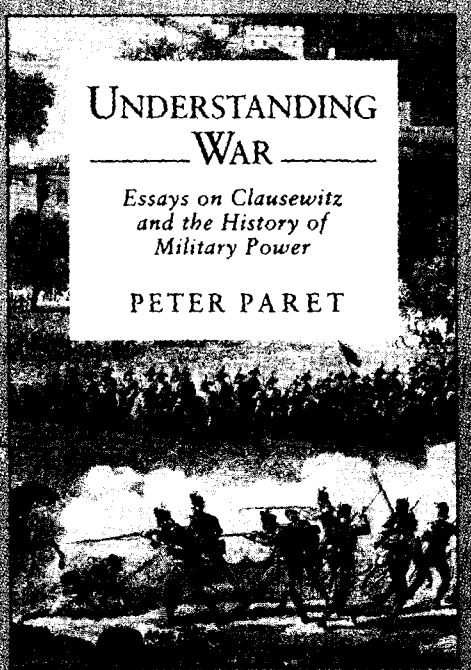
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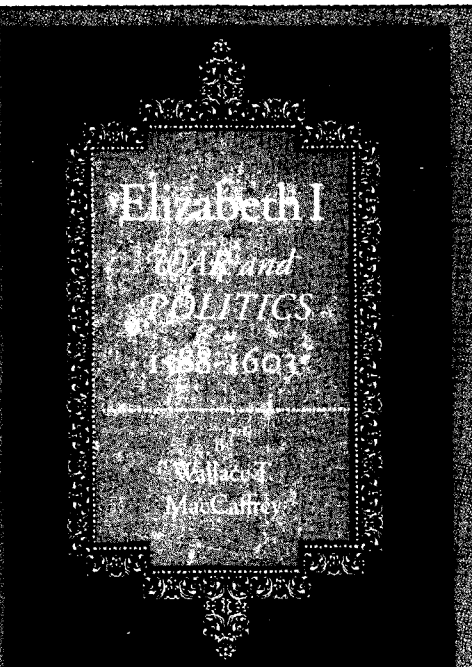
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